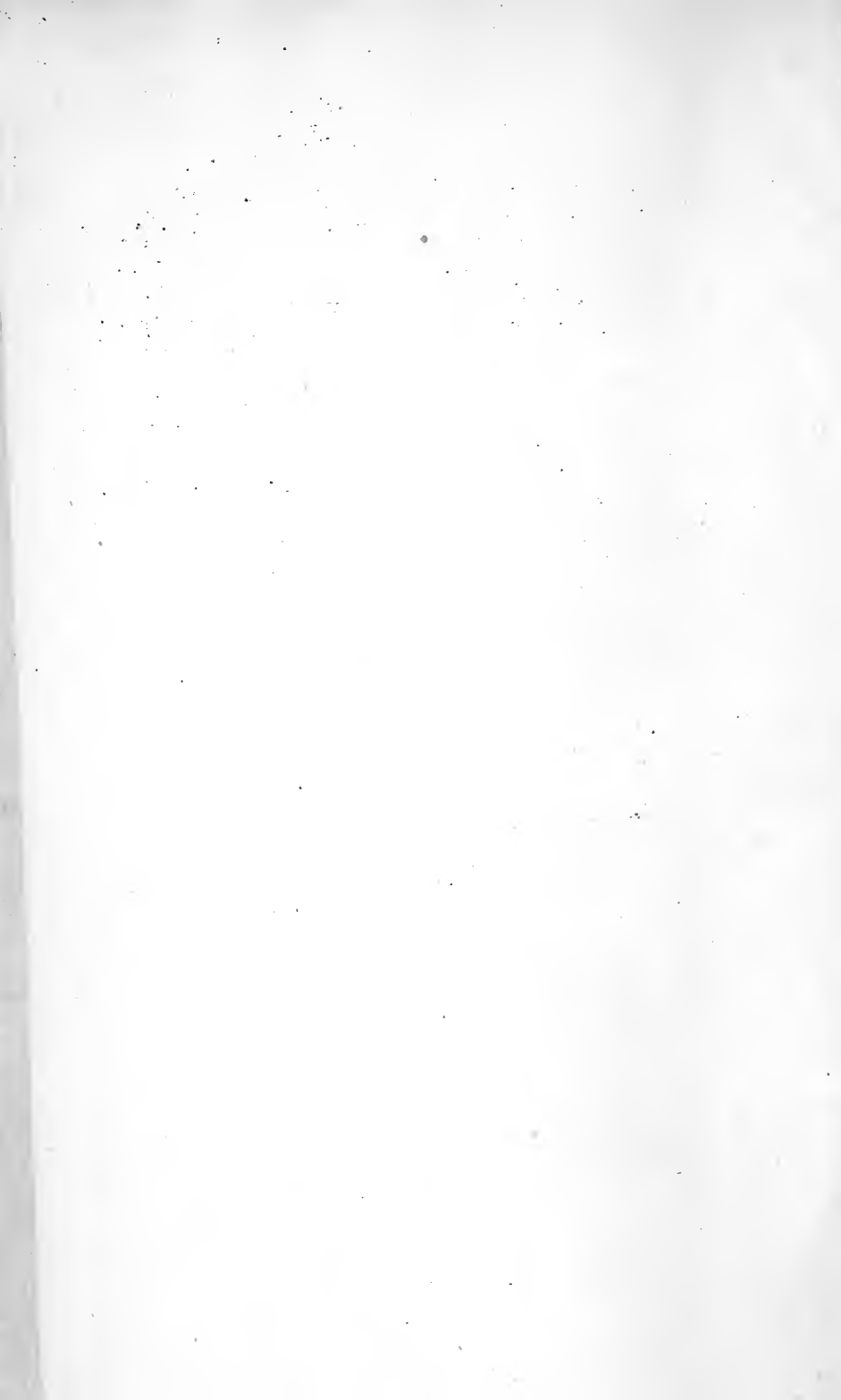




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JOURNAL OF THE TRANSACTIONS
OF
THE VICTORIA INSTITUTE.

VOL. II.

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JOURNAL OF
THE TRANSACTIONS
OF
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OR
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Second.—To associate together men of Science and authors who have already been engaged in such investigations, and all others who may be interested in them, in order to strengthen their efforts by association ; and by bringing together the results of such labours, after full discussion, in the printed Transactions of an Institution, to give greater force and influence to proofs and arguments which might be regarded as comparatively weak and valueless, or be little known, if put forward merely by individuals.

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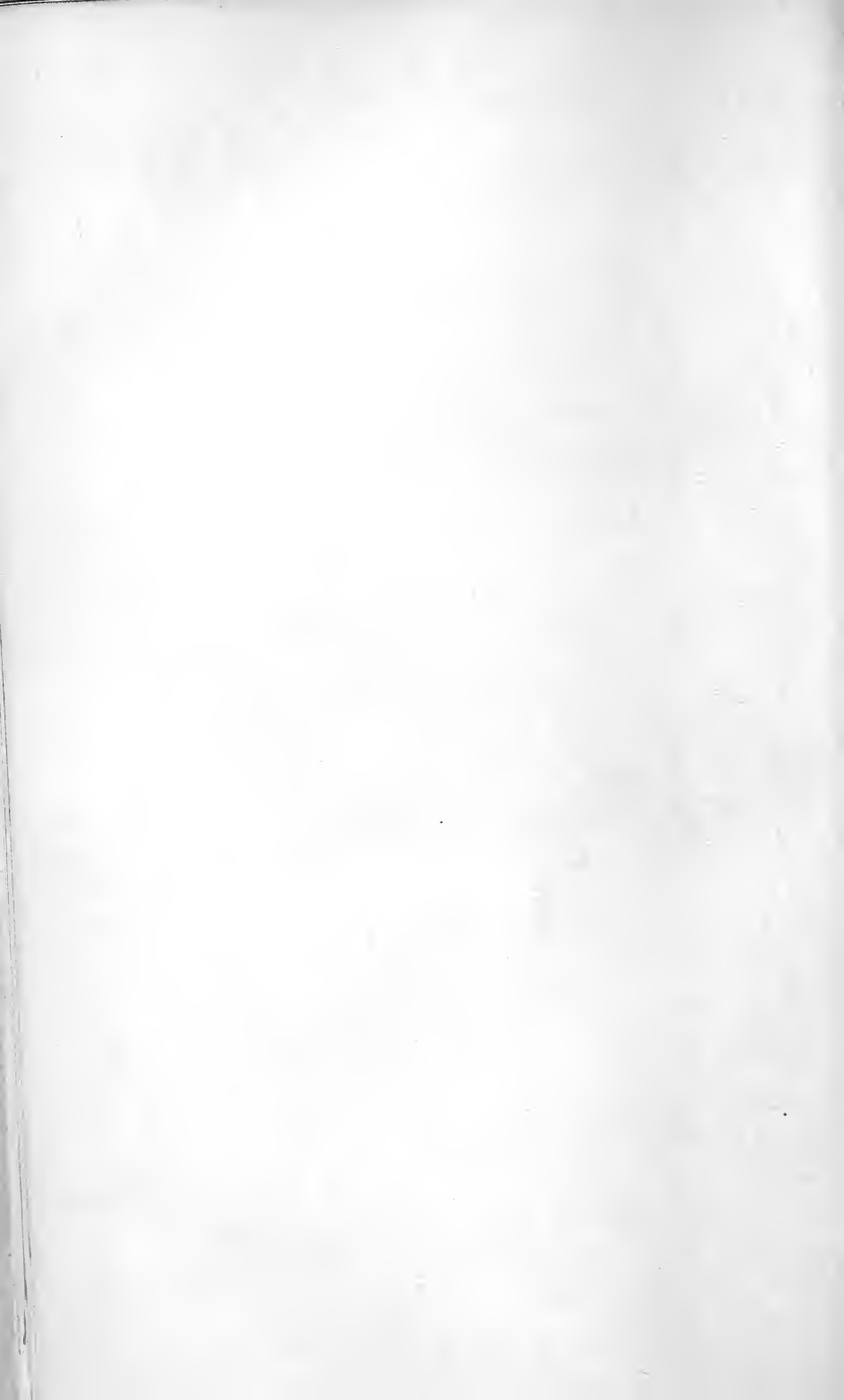
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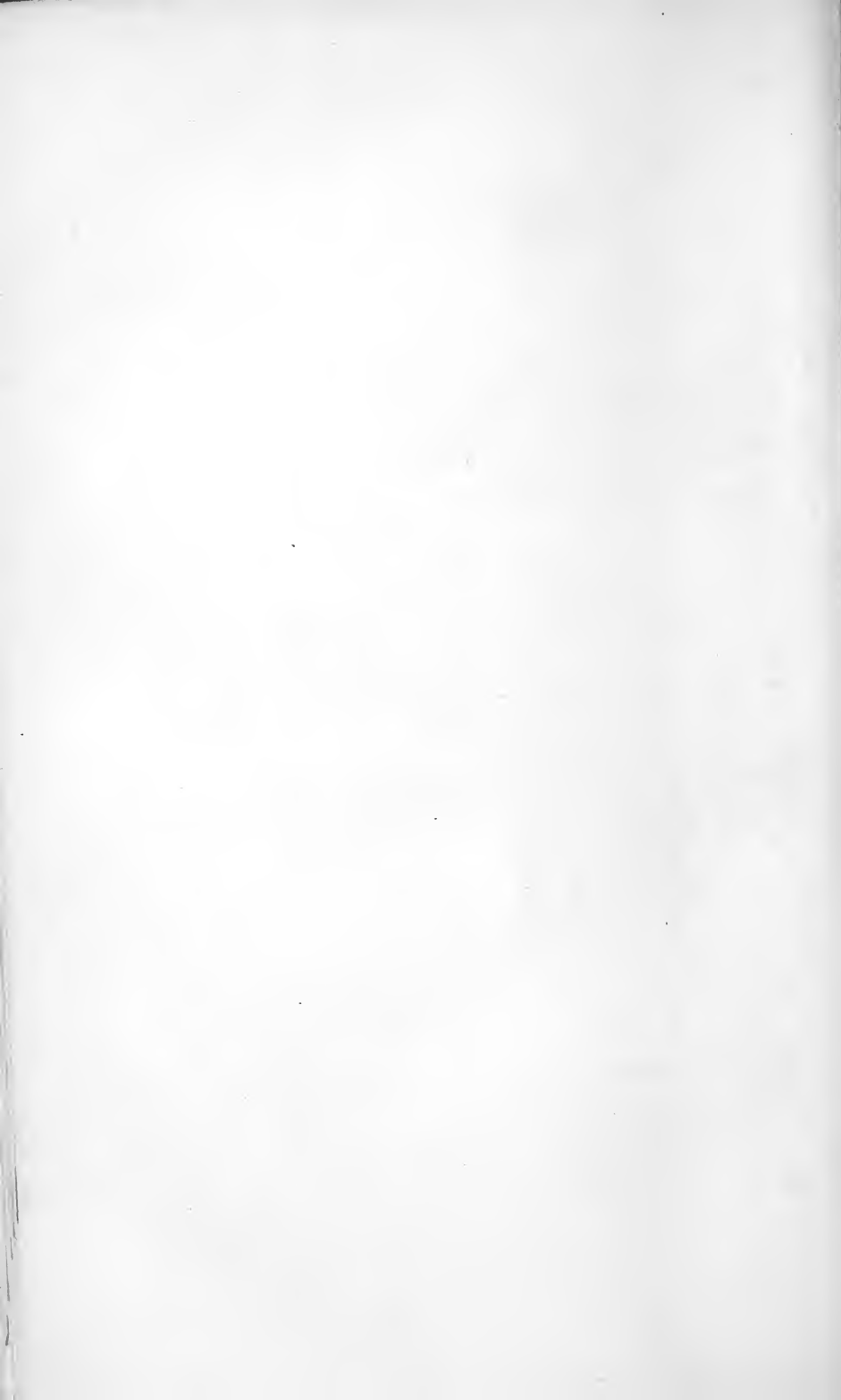
PREFACE.

IN issuing the second volume of the *Journal of Transactions of the VICTORIA INSTITUTE* but few prefatory remarks are necessary.

The volume will be found to contain several valuable Papers, not the least important amongst which is that on the Geometrical Isomorphism of Crystals, and the Derivation of all other Forms from those of the Cubical System, by the Rev. WALTER MITCHELL, M.A., V.P.

As regards the Institute's progress during the past year, we need but say that it has been all that could have been expected, considering the short time that has elapsed since the foundation of the Society.

THE EDITOR.



JOURNAL OF THE TRANSACTIONS

OF THE

VICTORIA INSTITUTE,

OR

PHILOSOPHICAL SOCIETY OF GREAT BRITAIN.

ORDINARY MEETING, FEBRUARY 18, 1867.

THE RT. HON. THE EARL OF SHAFTESBURY, K.G., PRESIDENT,
IN THE CHAIR.

The minutes of the previous meeting were read and confirmed, after which the following Paper was read by the Honorary Secretary in the absence of the Author:—

*ON TERRESTRIAL CHANGES, AND THE PROBABLE
AGES OF THE CONTINENTS; FOUNDED ON
GEOLOGICAL OBSERVATIONS AND ASTRO-
NOMICAL DATA. By EVAN HOPKINS, C.E., F.G.S.,
Mem. Vict. Inst.*

NOTWITHSTANDING the facts explained by geologists with regard to terrestrial mutations, the generality of mankind get so accustomed to and familiar with the configurations of our continents, during the comparatively brief period of their lives, that they look at them as they do at an artificial globe, and imagine that they have been the same since the days of Adam. The changes of the earth are so slow in comparison to the duration of our lives, that they are overlooked and forgotten. From the apparently quiet and regular succession of natural events to which we get accustomed, and the

repugnance we feel to the idea that it is possible for the foundation of our habitation to be always changing, upheaving, depressing, and moving *en masse*, from clime to clime, without our being sensible of such movements, we are apt to attribute all changes to past ages, and deny the possibility of their going on during the period of our existence.

Pythagoras taught, 2,350 years ago, that "the surface of the earth was ever changing; solid land had been converted into sea, and sea changed into dry land. Marine shells were found far distant from the deep, and the anchor had been found on the summit of hills. Peninsulas had been separated from the main land, and had become islands." "The changes of the earth," says Aristotle, "are so slow in comparison to the duration of our lives, that they are overlooked; and the migrations of people after great changes, or their removal to other regions, cause them to be forgotten. The distribution of land and sea does not endure throughout all time, but it becomes sea in those parts where it was land; and there is reason for thinking that these changes take place according to a certain system, and within a certain period. Everything changes in the lapse of ages."

Whatever difference of opinion may exist as to the cause of the upheavals and subsidences of the lands, there can be none as to the fact of their occurrence. These terrestrial changes are now too well established to be controverted; the observed facts must be and are accepted. My object on this occasion is, not merely to confirm them, but to point out the order in which they occur, with the view of forming some idea of the probable ages of the existing continents.

Various attempts have been made to compute the ages of geological formations, or the deposits of drifts with organic remains, by means of the rate of *upheaval* measured on any given coast at a certain time. Simple reference to the changes daily going on would at once show that such a method could only be adopted for a short period, within comparatively narrow limits, as the rate of rising and sinking is extremely variable, not only in countries far apart, but even along the coast of the same island. The western coast of South America, from Terra del Fuego to Panama, is subject to very irregular upheavals and depressions. So are New Zealand and Australia. Besides the slow normal mutations of the earth, there are also periodical actions of increasing intensity occurring during earthquakes, as on the coast of Chili in 1835, when the island of Santa Maria was upheaved ten feet in one day. The coast of Puzzuoli, near Naples, in 1538, was raised twenty feet in a single night. Therefore it is quite clear that no reliable data

for computing the age of any given land or formation can be obtained from *upheavals*.

I need not refer to the slow upheavals along the coast of Greenland, Norway, and Sweden, as they are neither uniform nor continuous in their movements. Lands often rise gradually for a certain time, then remain stationary at the same elevation above the sea, and again subside. No computations can be founded on such irregular and uncertain mutations. I alluded to the *upheavals* in Australia in my former paper, to which I beg reference. I shall next refer to another movement of the earth which has not been duly attended to, although it is the most important of all the changes; viz., the movement *northward*, which produces climatal and *geographical changes*. The evidence of the lands having not only upheaved, but also moved *en masse* from the tropics to the Arctic region, is as strong and conclusive as the proofs of their having been raised from the deep.

Before proceeding to consider in detail the northerly movement of the lands, I shall give a brief description of the currents of the ocean, and endeavour to show that terrestrial matter generally is subject to the same law of movement from pole to pole. The currents of the ocean are well described in Captain Maury's *Physical Geography of the Seas*, to which I beg reference for details. These currents commence in the Antarctic region, and after flowing along the various configurations of the coasts in the Indian, Pacific, and the Atlantic Oceans, terminate in the Arctic Circle, and become absorbed therein. These oceanic streams carry with them the vegetable forms of the southern climes into the Arctic basin, by means of the Gulf-stream in the Atlantic, and the Japanese stream in the Pacific.

The northerly actions of the "Gulf" and the "Japanese" streams are so well known as not to require further comment on this occasion. The actual rate at which the ocean moves, as a whole, from south to north, is not yet ascertained; but there are strong reasons for believing that the entire ocean changes place in less than seven years.

A bottle thrown into the sea off Cape Horn in 1837 was picked up on the coast of Ireland a few years afterwards.

This northerly action of the ocean alone causes very important geological changes, inasmuch as it not only carries the *débris* of the vegetation of different climes to the northern hemisphere, which become deposited in high latitudes, but it also conveys a large amount of fine sand and mud, held in suspension, from the mouths of great rivers (like the Amazon

and the Orinoco) northward, or in the direction of the local bends of the oceanic streams.

Had the vegetable tropical remains, which are found in the formations of the northern hemisphere, consisted simply of broken fragments and mere *débris*, their occurrence at high latitudes might easily be accounted for by the northerly action of the ocean; but as we also find tropical fossil trees standing with their roots still attached to the soil in which they grew, surrounded with their fallen leaves and the remains of reptiles, we cannot come to any other conclusion than that the lands themselves have moved bodily in that direction.

The oceanic streams radiate from the south pole as the fountain-head, and carry with them the sands, gravels, and the icebergs of the Antarctic region to very low latitudes, sometimes approaching the boundary of the southern tropic. Hence this part of the globe is a scene of desolation and barrenness to the parallel of about 50° latitude south. A very different appearance is seen in the north. The streams of the ocean flowing through the tropics enter into the Arctic basin comparatively warm, with floating vegetation from all climes, and become absorbed therein. The crust, or the crystalline film of the earth, has a similar action, but at a much slower rate of movement.

In Greenland, Spitzbergen, Iceland, Northern Canada, and Nova Scotia, we have excellent examples of fossil trees, in upright positions, with their roots still attached to the soil in which they grew; thus furnishing most incontestable proofs of their having flourished and died on the very same lands as those in which they are now found. At Atanekerdluk, in lat. 70° , trunks of trees are seen standing upright in their native soil. This fossil forest grew on the ground on which the plants are now found fossilized. The fossil plants of North Greenland prove that the land had been favoured with a climate at least 30° Fahrenheit warmer than it is at present, as it is quite certain that they never could have borne a low temperature. If we look at those species which we may consider as possessing living representatives, we shall find that, on an average, the highest limit attainable by them, even under artificial culture, lies about 14° to the southward. In Spitzbergen, lat. 78° N., we find the beech, hazel-nut, and some other species, identical with those from Greenland. The extreme northern limit of the growth of such plants as the fossil trees of Greenland is lat. 53° N. The conclusions drawn from the general appearance of the fossil forests of Greenland, are, that the country was, some years ago, truly a *green* land, on which vegetation flourished as abundantly as we now see in California. According to the Ice-

landic histories, Greenland had a large population in the year 982, with whom Hamburg as well as Norway merchants had a commercial intercourse. The communication continued till the year 1418, when, from the increased severity of the climate, and other causes, such as the black death, &c., the country and its inhabitants became forgotten, and almost disappeared from history.

The contents of all the European deposits indicate that in past ages the lands which are now in the northern hemisphere were once in a much warmer climate, and of so uniform and mild a temperature, that the surface was clothed with coniferous trees, arborescent ferns, and palms. The shores also teemed with turtles and various amphibious reptiles.

The Malay Archipelago is about the size of Europe from the North Cape to the Mediterranean, and from Britain to Russia, and therefore equals all the geological formations which have been examined by geologists with any degree of accuracy. This Archipelago, with its numerous large islands, contains the representatives of nearly all the organic remains found in the formations of Europe. I have seen sharks near Java upwards of twelve feet long, and chambered shells of large dimensions; also elephants of gigantic size in Ceylon and Malacca. The temperature of the sea is high; the nautilus and spirule Peronii, like the ammonites, and various mollusca, abound on the shores, and the corals grow in luxuriant clusters to great magnitude on the reefs, and the bottom of the shallow channels.

THE RATE OF THE TERRESTRIAL CHANGES DETERMINED UPON ASTRONOMICAL DATA.

At the commencement of the last century our geographical maps were extremely imperfect, therefore we have no reliable data on which to make a correct comparison as to the position of any given place now, and that which it occupied a few centuries ago. Humboldt very justly remarked that the latitudes of even the European observatories in the last century were not correct within *twenty minutes of a degree*. If this was the case in scientific stations, where correct astronomical data might have been expected, what must be the errors and the uncertainties of the positions of other places? These facts show that there are no grounds whatever on which it can be maintained that the latitudes of the lands do not change.

The only means by which we are able to ascertain the

latitude of any given place with exactitude is by reference to the fixed stars, as it is impossible to make terrestrial measurements from the poles. Even in taking the stars as the fixed points to determine the latitude at any given period, they must be observed when in the zenith, so as to be free from errors. The laws of refraction, even at the present day, are not sufficiently accurate for taking observations to determine small changes many degrees below the zenith; therefore, all computations requiring great exactness are founded on zenith distances.

Bradley, during his astronomical observations between the years 1726 and 1735, found, by comparing the catalogues of stars made by Hipparchus and Tycho Brahe, that a change had taken place in the position of the fixed stars with reference to any given station on earth, equal to 50 seconds of a degree per annum, in the plane of the ecliptic westward. Bradley made his observations by means of a vertical telescope. The star chiefly made use of to determine this change was that marked γ in the constellation of *Draco*. (See *Phil. Trans.*, 1748.) Besides the direct and continuous change of 50" per annum, he also detected a small undulating movement, which he attributed to a *nutation of the poles*. He was under the impression that the land was fixed to the globe, as the maps are on an artificial globe. Hence all changes were attributed to nutations of the axis, or to the earth bodily, and not to any movements of the surface of the globe. These movements, although well known in the time of Pythagoras and Aristotle, appear to have been forgotten, and therefore were totally neglected by modern astronomers, in speculating on the probable cause of the above changes.

Bradley was appointed Astronomer Royal in 1742; but beyond the fact of determining the annual change referred to, he made no further observation with reference to this question.

Had this movement only affected the fixed stars, it might have been urged that it originated from the starry heavens moving slowly towards the south-east, and not the lands towards the north-west. But since it also affects the sun, moon, and planets, such an idea could not have been maintained; therefore astronomers have necessarily concluded that it proceeds from a *real motion of the earth*. The correctness of Bradley's observations was subsequently verified. This change—which causes the appearance of a recession in the equinoxes, is so well established now as not to require further confirmation. The annual amount of this spiral movement of the surface westward at an angle of about $23^{\circ} 30'$ from

the plane of the equator, is but an extremely minute quantity, viz., $50''$ in longitude and $20''$ in latitude; yet its continual action from year to year makes itself very conspicuous, and that in a way highly inconvenient to practical astronomers. It destroys, in the lapse of a moderate number of years, the arrangement of their catalogues of stars, with reference to the stations on earth, and renders it necessary to reconstruct them from time to time.

Since the earliest catalogue on record—that made by Hipparchus 2,140 years ago—the stations of reference have moved towards the north-west 30° , and have, in round numbers shifted northward during the same time 12° in latitude. That is equal to the cosine of the angle of the spiral plane (of $23^\circ 30'$) the direction of the superficial movement. The effect of this change in the aspect of the heavens is to make the southerly stars appear to recede southward, and those situated in the north to approach at the rate of $20''$ per annum in the meridian. Hence it appears that the superficial film of our globe has been made free to move, like the ocean, from south to north, but in a spiral path: this movement has been determined to a fraction of a second of a degree, and is seven and a half furlongs in longitude W. and three furlongs in latitude N. per annum.

As a further illustration of this terrestrial change, let us, by way of an example, take γ Ursæ Majoris as a convenient fixed star to determine annually our geographical position. The situation of this star is very favourable for making observations in this latitude, inasmuch as it passes within 3° of the zenith, and therefore is, when in that position, unaffected by refraction on its transit.

In January, 1853, Greenwich was $3^\circ 2' 5''$ to the south of the transit of γ Ursæ Majoris. In January, 1864, the Observatory was $2^\circ 58' 24''$ S. of this star. In 435 years hence the Observatory will have arrived at the same parallel as γ Ursæ Majoris, when the star's transit will be seen in the zenith. It might be urged that such a small movement, which is only detected after the lapse of ages, would not be sufficient to account for the geological changes referred to; but I shall endeavour to show that, small as it is, it is quite sufficient to produce them, and in the exact order in which they are seen. I shall take the Isle of Portland as an example. In the deposits of this island is a petrified tropical forest, proving that that part of England has not only been upheaved, but also exposed to a tropical, or at least, a semi-tropical sun. Many of the fossil trees are still standing erect, with the roots in the very ground in which they grew. The plants are similar to

the palms and other varieties of vegetation now flourishing luxuriantly in Africa in latitude 35° N. Dr. Hook, in 1705, remarked that "the fossils found in Portland seemed to him to have been the productions of hotter countries; and it is necessary to suppose *that England once lay under the sea within the torrid zone.*"

Let us compute backwards how long ago it is since the south of England was in latitude 35° N., where the animals and vegetables found entombed in the Portland deposits still flourish. As we are now in possession of the exact amount of the rate of the terrestrial change, which has been determined to a fraction of a second, we can safely proceed with our computations, and thus ascertain, with some degree of exactitude, the probable age of any given land. About 3,150 years ago, the site on which Greenwich Observatory stands was about $20^{\circ} 28' 30''$ S. of the parallel of γ Ursæ Majoris, and therefore in latitude 35° N., when the Portland organic remains might have flourished, and the deposits have been formed. The southern part of England, according to the slow rate of change of $20''$ per annum, must have been within the tropics about 5,500 years ago. Hence England might have risen from the deep within the tropics, and produced all the geological deposits found on it during the last 6,000 years, without allowing for any increased movement, which it is highly probable occurred during the Noachian deluge.

Amongst the animals entombed in the deposits of Siberia are the elephant, rhinoceros, hippopotamus, bear, hyæna, lion, tiger, and others, which can only live and flourish in or near the tropics. The fossil ivory is found in deposits like quarries of bones, and forms a lucrative article of commerce. Tobolsk, the capital of Siberia, is now situated in the parallel of 58° latitude N.

In Cabool, Lahore, and Delhi, say in 30° latitude N., elephants and tigers still abound. How long is it since Tobolsk (site) was in the parallel of 30° N. latitude? The difference between the two parallels being 28° , the time required to produce this change is 5,040 years.

The flesh of the Siberian mammoth has been found in the ice and gravel in so fresh a state as to serve as food for dogs, bears, and wolves. Yet it is contended that the deposit *must* be tens of thousands of years old!

There are species of tigers and other tropical animals roving occasionally as far north as 45° . A tiger was killed in 1828 on the Lena, in latitude $52\frac{1}{2}^{\circ}$ N. Bears, with long hair, and black tigers, are seen within the tropics, as high as the inferior limits of perpetual snow; therefore these animals are not

necessarily confined to the tropical parallels. The long-hair mammoth found in Siberia might have lived far beyond the tropics, and the carcass might have been carried by the floods of the rivers towards the Arctic region two or three thousand years ago. Had Africa been connected with the south of Spain, as it was in former ages, Spain would even now be infested by tropical animals. It is necessary to bear this in mind when we discuss questions connected with terrestrial changes. The African rhinoceros is found as far south as the Cape of Good Hope, latitude $34^{\circ}30'S.$, and lions as far north as Algiers, about 36° lat. N.

The fossil forest of Atanekerdluk, in latitude 70° N. (Greenland), is still standing erect on its native soil. When those trees flourished, they required a temperature of at least 30° Fah. higher than is now found in that parallel. This land 4,000 years ago was within the parallel of 48° N., in which similar vegetation now flourishes in France.

The Nova Scotia coal-beds contain calamites, fern-trees rooted in the arenaceous beds, surrounded by their fallen leaves, and the remains of tropical reptiles. This formation is now in latitude 45° N. About 4,000 years ago it was in latitude 23° N., and might have then received its sedimentary deposits, in the same manner as they are now seen forming in the lagoons of St. Martha, near the mouth of the river Magdalena.

The south-east part of England, when the Wealden deposits were formed, was in a very warm climate. It had then its lagoons, with palms, arborescent ferns, &c. Crocodiles, iguanoes, turtles, and various reptiles, infested its fens and rivers, and have left their remains as memorials of their former existence. All this might have occurred about 4,000 years ago, when the south of England was in latitude 30° N.

How much more satisfactory it is to the inquiring mind to learn that these great geological changes are not the result of chance or disturbed elements, but are occurring as regularly, and are as uniform and exact in the rate of their movements, as the rotation of the earth; and that they do not proceed from a series of igneous catastrophes, regulated by no laws, and reducible to no fixed principles, as assumed by geologists.

I shall not refer to the theory which was propounded at the commencement of the last century, and attempted to be improved by D'Alembert, to account for the change referred to by an assumed conical motion of the terrestrial axis. This inconsistent hypothesis has been lately exposed and demolished by M. Poinset, an eminent member of the French Insti-

tute. Astronomers will continue to be perplexed with the results of their observations until they have corrected their tables of refraction, and adopted the now well established superficial movement, instead of the *reeling* or *conical motion of the terrestrial axis*, which has not a single physical fact to support it. The Astronomer Royal, in his report for 1861, remarked that "the transit circle and collimators still present those appearances of agreement between themselves, and of change with respect to the stars, which seem explicable only on one of two suppositions—that *the ground itself shifts with respect to the general Earth*; or that the axis of rotation changes its position."

We have innumerable proofs of the land's upheaving, subsiding, and shifting, but none whatever as regards any changes in the position of the axis.

I shall next refer to the lands of the southern hemisphere. The conditions of that part of the globe are very different to those in the northern hemisphere. With the exception of a small part of the south of New Zealand and Patagonia, there are no lands in the Antarctic Sea, but mere patches of sands, gravel, and icebergs—scenes of barrenness and desolation—to the latitude of 45° S. In the north, between the parallel of 45° latitude N. and the Arctic basin, are situated all Europe as far as Spain and Italy. Also Siberia, Tartary, and the northern part of China. Likewise nearly all the British possessions of North America. We have to advance from the south as far as the tropics—say to the latitude of 20° S.—before we can obtain sufficient area of habitable lands in that part of the globe to investigate their geological formations, so as to form a correct opinion of their general character and probable ages. The parallel of 20° lat. S. will embrace New Zealand, Tasmania, about two-thirds of Australia, the Cape of Good Hope, and the southern part of South America. A general description of the deposits of these countries has been already given, therefore need not be repeated on this occasion. In the sedimentary deposits of the northern hemisphere are the remains of the flora and fauna of the semi-tropical and tropical climates, and not those flourishing in the northerly zones in which the deposits are now seen. The relics of the past entombed below are totally uncongenial to the climates of high northern latitudes.

In the south, on the contrary, *the organic remains found in the deposits correspond with those now living in the same regions*. In Australia, New Zealand, and the southern part of South America, are growing most luxuriantly arborescent ferns, Cycadeæ, Araucariæ, and various coniferæ. The coasts abound

in corals and sponges even to Tasmania; also Terebratula, Trigonia, and a variety of mollusca, unknown in Europe excepting as fossils. Hence the entombed organic remains of that part of the world present no indications whatever of the lands having shifted from other climes, as we see in the northern hemisphere. We have many unequivocal proofs of the comparatively modern origin of the lands of the southern hemisphere, and that they gradually emerged from the sea, but they are occasionally subject here and there to somewhat rapid upheavals, as seen in New Zealand and on the coast of Chili, as described in my former paper.

To determine the probable age of the lands of the south temperate zone, we can have no assistance from the organic remains; we can only be guided by astronomical observations and the space traversed from the parallel of emersion in the south to the parallel now occupied. However, this is quite sufficient for my object, and as the fact of the 20" per annum movement has been well proved in all parts of the world, and, indeed, is recorded in the *Nautical Almanac* as an essential element to be taken into account in all our astronomical observations to insure accuracy, the ages of the lands referred to can be determined within a few centuries.

I shall take the southern part of Australia to the latitude of 30° S. as an example; and as this parallel will embrace also the whole of New Zealand, Tasmania, Cape of Good Hope, Patagonia, La Plata, and Chili, the computation will serve for all. The result of the various explorations which have been made in the Antarctic Sea shows that *no permanent land on which animals and plants can exist* emerges and remains above the level of the sea until it reaches the parallel of about 50° lat. S. All the so-called lands, with the exception of Terra del Fuego and its neighbourhood (which is a hard mass of primary rock), are mere shifting sands and gravels, constantly subject to be washed away by the streams and icebergs flowing from the south polar region towards the north.

If, then, we take the parallel of 50° as the starting-point from which Australia and the other lands mentioned emerged from the deep, it follows that, according to the rate of movement of 20" per annum in the meridian, the lands bounded by the parallel of 30° lat. S. can only be 3,600 years old at most. They might have first risen from the deep in the parallel of 45°, which, indeed, is highly probable, as they contain but a small amount of terrestrial deposits, and predominate in loose sands and gravel with marine shells, indicating comparatively recent origin; this would reduce the age to about 2,700 years. According to the present rate of rising in Australia, four-

fifths of that country were below the level of the sea 1,000 years ago. In making these computations on the probable ages of the existing lands, it must be borne in mind that the computations refer strictly to the dry lands or continents, and not to the earth as a body. The globe, with all its elements, might have existed from eternity. The ocean and the lands emerging from the Antarctic Pole, merge again into the Arctic Pole, and thus circulate from pole to pole through the medium of the earth's axis. This question is beyond the reach of demonstrable science; but as regards the existing dry lands, we are able to determine the extreme limits of their probable ages almost to a mathematical certainty.

A new land emerging from the deep in latitude 50° S., moving at the present slow rate of 20" per annum northward, would arrive at the Arctic Circle in less than 22,000 years. Hence, had Greenland been emerged in that parallel, and had since been slowly shifted from thence, it could only be 22,000 years old. But as far as the fossil contents of that country are concerned, Greenland might have emerged from the sea, like many other northern lands, in latitudes corresponding to Spain and Portugal, and if so, it might not be above 5,000 years old. Again, we must not forget the miracle of the Flood. It is highly probable that the Flood was brought about by means of the established terrestrial physical operations. The movement of the ocean northward must have been greatly *intensified*, and thereby, from the same natural causes, the action of the lands in the same direction, must have increased, and thus, during their immersion by the ocean, have been carried *en masse* many degrees northward, with the carcasses of the animals then destroyed, leaving a new land for Noah and his live stock free from the remains of the former animals, to replenish the world with organic life for future generations.

Besides this possible extra movement northward during the Flood, we have to reflect also on the great intensity of the action of terrestrial operations in the days of the Creation. When everything was created and made to appear perfect and in a state of maturity, as quick as the word of command, "Let it be; and it was so" — *time* was not required. Although the Creation was spread and divided over six days as a type of certain ordinances which were to be established for the guidance of man, so many days could not have been required by our Maker. The great *intensity of action* which of necessity must have been going on during the days and nights of the Creation, has not been sufficiently considered by those who have attempted to compare geological formations with the Mosaic records. Greater results must have then been pro-

duced in an hour than were effected, subsequently to the sixth day, by the normal action of the polar forces in many centuries. Why then demand a greater time than is recorded in Genesis, and declared in the fourth commandment? The lands must have risen above the level of the sea on the second day within the tropical zone, to provide the necessary vegetable nourishment for the animals which were to appear on the fifth and sixth days. The oceanic movement and the electro-magnetic currents of the globe from the first day to the fifth, before man and the large terrestrial animals appeared on the scene, were doubtless circulating from pole to pole at a rapid rate, preparing and forming the entire surface of the earth in every zone from south to north.

Taking all these terrestrial operations into account in connection with the established fact of there being a constant movement on the surface of the earth equal to 20" per annum in latitude northward, I have long come to the conclusion that there is no necessity whatever to alter the literal meaning of the first chapter of Genesis, or the description of the Deluge, nor yet to alter the Jewish chronology, in order to account most satisfactorily for all geological phenomena hitherto discovered.

The great electro-magnetic power which envelops our globe circulates from pole to pole, and completes its circuit of action through the medium of the axis. It propels the currents of the ocean from the Antarctic to the Arctic focus of convergence, and by its directive property and action on magnetic needles guides the mariner on the seas in the darkest nights. The existence of this great universal power was scarcely known a few centuries ago; yet its everlasting action in the subterranean base imperceptibly changes the aspects of man's habitation, remodels again and again the superficies of the globe, and makes all pass away in succession like a scroll. Thus all things terrestrial are ever changing, decaying, and renewing; the lands, like generations, are passing through different stages, and finally merge into eternity, according to the will and ordinance of our Maker.

The CHAIRMAN.—It is my duty, as President of the evening, to propose a vote of thanks to the able and accomplished author of this paper. You will hardly expect from me anything in the shape of scientific criticism; but I may say this—and I have no doubt that many of you would say the same—that I have been intensely interested by what we have heard. I think it is an admirable paper, and one that must be productive of benefit to us all. Without entering into discussion, I will mention one fact that may be regarded as interesting. I could not help being struck by the frequent allusions in this paper to periods of 4,000, 5,000, and 6,000

years ; and when Cuvier was in England, I saw him frequently, and one Sunday evening I was with him, and whilst we were talking of the Bible and modern science, he said this :—" All my researches have brought me to this conclusion, that the geological changes on the earth do not require a longer period for their accomplishment than 6,000 years"—the period which we think is the duration of the world from the beginning, as we gather from the first chapter of Genesis. I will now invite discussion on the paper, and any gentleman who has anything to say will please address the meeting.

Mr. WARINGTON.—Before I commence my remarks on this paper I wish to ask one question. I was in hopes that the author would have been here to answer it, but I dare say, though he is not here, some other gentleman better acquainted with astronomy than myself may be able to solve my difficulty. It is this. Mr. Hopkins states that the direction in which the crust of the earth is moving, is at the angle of $23\frac{1}{2}$ degrees to the Equator, that is to say, in the same angle as the line of the ecliptic ; and he says that this is equivalent to an annual motion in latitude of 20 seconds, and in longitude of 50 seconds ; in other words, the proportion is as two to five. Now, upon looking at the globe, and seeing what relation there is between the changes of latitude and longitude involved in the motion of the ecliptic, I find instead of these changes being in the proportion of two to five, they are in the proportion (nearly) of two to eight. How is this to be explained ? Which is right ? Is the motion really a motion in the plane of the ecliptic at an angle of $23\frac{1}{2}$ degrees, or is it a motion in the proportion of two of latitude to five of longitude—that is, at an angle of 36 degrees ? I want to use these figures in testing Mr. Hopkins's conclusions, and until I know which method of reckoning is right I am altogether at sea. Is there any one present who can help me ? If so, I should be glad if they would do so before I say another word.

Rev. WALTER MITCHELL.—I think, perhaps, Mr. Warington may be labouring under a misconception. There is some degree of vagueness on that point in the paper ; but I think that astronomers admit there are two motions, or one motion, in reality, which is resolved into two. One of these motions is accounted for by a gradual change of the point at which the ecliptic cuts the Equator. That is the motion by which the plane of the earth's motion round the sun is slowly changing ; but that is not sufficient to account for all the changes. Besides that, which is called the precession of the equinoxes, there is another change, and that is accounted for by what is called " nutation," consisting of a wriggling motion of the earth's axis, as it were, in space. While the plane of the motion is changing, you have a change like the motion of a teetotum ; and the whole change that takes place is compounded of these two motions. It was the popular theory a little while since—the generally received theory of all the text-books on astronomy—that there was no real motion of the earth's crust, but that the only motion was a change in the earth's axis occasioned by the disturbing forces of the moon and planets upon the earth. There is now a growing belief on the part of astronomers, including the Astronomer Royal, that the

above causes are not sufficient to give the explanation of the whole motion ; and now there is a tendency—(and it is so far admitted as to be discussed by the Royal Society, and it has been entered into by other authors than Mr. Evan Hopkins)—to assert that there is in all probability an actual motion of the earth's surface ; but as to matters of detail or calculation as to this movement, I am not at present prepared to enter into.

MR. WARINGTON.—My course must be, then, to take Mr. Hopkins's figures, rather than his angle, since it is the figures, not the angle, which he uses for his calculations. Now, let us realize the motion which this theory assumes. In the first place, it is a motion of the *whole crust* of the earth, of course only visibly apparent in the continents, but really extending over the whole surface of the globe. If, for example, England is moving in a certain direction, it is very plain that the bed of the sea on all sides must be moving also, or there would be a continual wrenching of the earth's crust going on where sea and land meet, such as we know does not, in fact, occur.

MR. REDDIE.—Mr. Hopkins considers the sea as included in the crust of the earth.

MR. WARINGTON.—Then, in the next place, it is a motion of the earth's crust to a considerable *depth* ; we do not know what depth, but it is certain, whatever the motion is, it is a motion which affects the earth to a considerable depth, not merely a surface of a few hundred yards, but a crust some miles (at least) thick of solid rock. What, then, is Mr. Hopkins's notion ? It is that of a spiral motion by which every portion of the earth's surface is perpetually, as long as the motion goes on, getting nearer and nearer to the North Pole. Bear that in mind. He supposes the land to start from the South Pole, to pass the whole way up northward to the Equator, and then on again to the North Pole. This is the theory as I understand it, and it is a motion strictly spiral, by which the whole crust of the earth is constantly tending northward. I ask, then, what mechanical alteration in the surface of the earth does such motion occasion ? You will observe that the earth being a sphere, the parts nearest to the poles are far smaller in circumference than those near the Equator. What, then, does this theory require us to believe ? Why, that this same identical thick crust of earth, which occupies now a certain space, is being perpetually crushed up together and put in a smaller space. For example, it requires us to believe that the land which stood in our latitude 6,000 years ago has passed on into a latitude 30 or 40 degrees further north, where it now occupies only one-half the surface it formerly occupied, since this motion is not only said to be taking place in England, but the whole surface of the globe is supposed to be thus tending northward. The theory involves, therefore, of necessity an enormous crushing together of the crust of the earth. (Hear, hear.) Is that a fact ? Let us take the change involved in our own latitude within a single year by way of example. I have made a rough calculation of what this would amount to, and find that the mere motion of a single year (if this theory be correct) involves a crushing of one mile and three quarters of the earth's surface into nothing—that is to say, in one year hence this solid crust of earth is to be

crushed together to such an extent that it shall occupy $1\frac{3}{4}$ miles less surface than now ; and this is to be continued year by year at a continually accelerated rate, since the further north the land proceeds, the faster will be the crushing. I ask, then, what change do we see going on, or can we trace historically, which can, in the smallest degree, answer to this crushing of the earth's surface, which is such an essential element in Mr. Hopkins' theory? Is there such a phenomenon? Now, you will observe there are only two ways in which this action can take place. It must be either by a crumpling and crumbling of the earth's crust, throwing it up and down, or it must be by a bending of the surface, as to cause it to occupy a smaller horizontal area. The first method may be rejected at once as incredible. Concerning the second it is to be asked, What amount of bending would be required? Suppose an extreme case, that by this bending the surface formerly horizontal was thrown into an angle of 45° ; this would only cause a diminution of about one-third in the original area occupied, and so, instead of $1\frac{3}{4}$ miles, we should require 5 miles of the earth's surface in our latitude to be yearly thrown from a horizontal position into an angle of 45° , to account for the change. Now we are certain, from what we know of the amount of rising and sinking actually in progress, that there is no such oscillation of the earth's surface—no such bending and doubling of the surface going on at the present time, as will account for this perpetual diminution of the surface. This is not all, however. In the northern hemisphere, you have this crushing of the surface together, but in the southern hemisphere you must have just the reverse—a perpetual extension and spreading out. The land in the southern hemisphere is supposed to be constantly getting nearer to the Equator, and so covering a larger surface than before, which involves, of necessity, a cracking and pulling of itself out. Now, solid rock, of the depth of several miles, is not easily pulled or stretched out, any more than it is not easily bent about or crushed. But even if this could be done—if the land was so peculiarly ductile as, in fact, it is not, still you have only got through half the difficulty ; for I ask next, When the land has got to the North Pole, what becomes of it? Here has been the whole crust of the earth, for the last 6,000 years, going to the North Pole. Where is it? It has not formed itself into a great mountain at the North Pole. Where is it? Observe this—it is not merely a crumpling up, or pulling out, year by year, of so many miles of the surface, but a pushing away of all the land that was there before. Mr. Hopkins refers, indeed, for analogy to the ocean ; but what do we find there? True, there are enormous currents of water passing from south to north, but, then, there are also equally enormous return currents, and without these return currents the motion could not take place. There is no great store of water in the south from whence a supply may be sent to the north, neither is there any gigantic vessel or receptacle at the north for the water to run into ; the water, to circulate thus, must get back again, and it does so. The question is, then, can the land, in like manner, get back again? Mr. Hopkins's theory plainly requires us to believe that it does. He says nothing of any accumulation of land at the North Pole, or

of any unfailing store of land at the South Pole to supply the place of the land moving northward. Yet, how is such a return current of land to be conceived? In the face of such difficulties; such a crumpling together of the land in the north; such a pulling out of the land in the south; such an utter lack of information as to where the land goes to, and from whence it comes: how, I ask, can we receive this theory of a spiral motion of the earth's crust? It is simply incredible, because of the mechanical difficulties necessarily involved, which mechanical difficulties would seem to have been altogether overlooked. Whoever, then, maintains this theory to be the true one, is bound to tell us how it is that the land coming from the south first occupies an immensely larger surface as it approaches the Equator, and then an immensely smaller one as it proceeds further north; what becomes of it when it reaches the North Pole, and whence it came from at the South; and, further, to give proof that such changes as these are, in fact, now taking place. It seems, then, that this idea of a spiral motion must be given up. Must we say, in consequence, that there is no motion in the surface of the earth to account for the observed astronomical changes? In making a hypothesis, we are bound to account for the facts of the case. Some motion there must be somewhere which accounts for these astronomical changes. What motion, we may ask, would account for this? If the whole surface of the earth were simply revolving round, not spirally towards the North Pole, but in a plane inclined to the Equator—that is, half towards the north-west and half towards the south-east—these changes of latitude and longitude would at once be accounted for, and this without any crushing together or pulling out of the land, or getting land from no one knows where; but to do this the motion must not be, as I say, a spiral one from south to north, but one of simple revolution in an inclined plane to the Equator. Such a motion will account for the facts of the case. Now observe, if we take this view, what follows. We have no longer a motion of the *whole* earth's surface to the north-west, but we have *half* the surface moving to the north-west, and *half* to the south-east; since, if the motion going on the whole way round, is of the nature of a revolution, each joint of the surface must eventually come back again to where it at first stood. It seems, therefore, that the only motion of the earth's crust which will account for this astronomical phenomenon (if in this way it can be accounted for) is a revolution of the entire crust, as of a hollow sphere, without crushing together and breaking, and that such motion must of necessity return upon itself. It is impossible to twist a revolving sphere in such a way that at last every point shall not return to its original place, that is always supposing you do not disturb the substance of the sphere itself. But if this is the case, how will it tally with the facts alleged as to changes of climates? Mr. Hopkins gives England as an example; and we can well suppose that England, moving in this way, was once in a much hotter climate than now—a tropical latitude, if you will. Then he takes another case, Greenland, and another, Australia. But it is simply impossible, on this view of the motion of the earth's crust, for all these three parts of the globe to have

been at the same time moving in a north-westerly direction. One or the other must have been moving in a south-easterly direction, since you cannot, by any art, make all three come within a single hemisphere ; and they cannot, therefore, on this view, have all been moving to the north-west. In one or other case, then, I do not in the least care which of the three, so far from these astronomical changes involving a change of climate which would account for the geological phenomena, they involve a change quite the other way. The theory does not then, even at its best, account for the facts of the case. Now to apply another and very different test. Hitherto I have been dealing with this subject on purely theoretical grounds ; now I want to come to the test of positive historical fact. I propose to test the worth of the theory by what we know of the ancient climate of Palestine, the land of which we have at once the earliest and the most authentic historical account. We know from the Scripture to a certain extent what the climate of Palestine was 3,400 years ago. Now, if Mr. Hopkins's theory is correct, if the land of Palestine has been moving at the regular rate of twenty seconds North Latitude, and fifty seconds West Longitude every year, then it follows that 3,400 years ago Palestine was not where it now is, but where Madras now is ;—that is, in the very heart of the tropics. If you look carefully to the evidence of the Pentateuch, you can prove to a certainty that there has been no alteration in elevation or general geographical situation in Palestine during the last 3,400 years ; you can prove that the sea-coast lay in the same place, that the mountains were of the same height, since the views seen from their summits then are the same as those to be seen now ; that the whole state of things, in fact, exactly corresponded with what we now see ; and we thus are not at liberty to assume any change of this kind to account for variation in climate. I ask, then, does Biblical evidence show us, that in the days of Moses Palestine was in the tropics ? Was the climate, then, such as it must have been if Mr. Hopkins's theory is true ? Let us look at the subject carefully. In the first place, we notice that the vegetation now observable in Palestine is identical with the vegetation mentioned in the Pentateuch. You have oak, the terebinth, &c., as the characteristic trees then just as now ; the palm, mentioned but seldom, and as found only in certain places, as in the Valley of Jordan, just as at present. In the same way, also, with regard to the zoology of Palestine, we know perfectly well—for it is one of the things we advance as proof that the Bible is authentic, that the plants and animals, the zoology and botany of the country at the present day are exactly those which the Bible describes. Is this credible, if a change of climate has taken place during the interval from the tropical climate of Madras ? But now, to bring this home to particular instances. It might be said, Ah, but these plants will grow also quite as well in the tropics. This is not the case ; some of them will not grow in the tropics ; and we have instances of such mentioned by Moses as growing in his days in Palestine. First of all, I will mention the olive. Humboldt says that the olive will not grow in the tropics. I suppose few men have studied more deeply the subject of the distribution of plants than Alexander Von Humboldt ; we may

well, therefore, take him as an authority. And he has laid this down, as the result of his researches, that the olive will not grow in the tropics. The land of Palestine and Egypt seems, indeed, to be the extreme southern limit in which the olive will grow. Now there is no doubt that the olive was one of the characteristic productions of Palestine. Moses describes it as "a land of figs, of pomegranates, and of oil-olives." There is thus no doubt that it was a characteristic production of Palestine in his days; yet this it could not have been, if Palestine was situated then where Madras is now. Then I take the case of wheat and barley. In the tropics, wheat and barley cannot be cultivated, because of the intense heat which dries them up before they have time to ripen. Look, for instance, at Johnston's "Physical Atlas," where he describes the different regions in which different cereals are grown, and you will find wheat and barley as the productions of the coast of the Mediterranean, Palestine, Greece, Italy, Spain, and the north of Africa. But the instant you get into the tropics you have not wheat, but rice. Now there is no mention of rice in Palestine, but there is of wheat and barley. Moses says it is "a land of wheat and barley," but of rice he says nothing. Wheat and barley, however, could not have been productions of the land if it experienced the same climate as Madras. Then again, we are told the wheat and barley will especially not flourish in hot climates if the land is flat and near the sea. Now, what were the characteristics of the grain districts of Palestine? Why, flat plains, and especially the flat plains of Philistia. Look at the history of Samson, and you find an allusion to this, where he tied firebrands to the tails of the jackals, and sent them into the standing *corn* of the Philistines. You see there the character of the place, standing corn growing on the land—land where it would be impossible for it to grow if Palestine had the same climate as Madras. But, further still, those persons who have been to Palestine, and examined most carefully its climate and productions, who are also most deeply conversant with the evidence of the Old Testament, tell us that so far from the evidence pointing to the climate of Palestine at the present day being colder than it was before, it tends the other way, that rather it was *colder* in the days of David and Moses than at the present time. And why? Why, because you find more mention of *snow* in the Old Testament than we should expect to have found from the present experience of the inhabitants. For instance, you have such an incident as that recorded in the days of David, where one of his mighty men went and slew a lion—on a snowy day. That is the very thing, the snow is referred to as a natural, common occurrence, and so is frequently introduced into the Psalms as an emblem of glory and purity. Now in the present day, snow is extremely scarce in Palestine, and therefore the probability is, that instead of a hotter, it had formerly a colder climate than at present. Let us take another step yet. Upon the ravines of the Lebanon there are plain marks of glaciers having once swept down them, and yet we are told that the climate formerly must have been enormously hotter than it is at the present day. Now when we test in this way, not by theory, but by taking a plain case

of real historical evidence, to show what the climate of a particular land really was three or four thousand years ago, it seems to me impossible to accept Mr. Hopkins's theory as true. I am not denying his facts, that the latitude and longitude, astronomically considered, have altered to the extent he says. I am looking at the matter from a purely practical point of view. Does that alteration of latitude involve a change of climate? and taking account of this instance of Palestine, I am compelled to conclude that it does not; for though the latitude may have altered astronomically, it has *not* caused a corresponding alteration of climate. What follows then? Granted that the latitude of Greenland, of England, of Australia have varied to the extent that he says they have, and I am quite prepared to admit it, still this does not involve what Mr. Hopkins would have us think it does,—the change of climate. The latitude has moved, but the climate we have no ground for thinking has thereby altered in the least. I am afraid I am taking up a great deal of your time (No, no), but I was interested in the matter, and have gone into it somewhat fully. Now just a few remarks on the subject of the time involved in Geological changes generally. What does history show us as to the period which has elapsed without any change in the surface of the globe having taken place? I take again the land of Palestine, or rather one remarkable portion of it, the Dead Sea. We can trace back the history of the Dead Sea to the days of Abraham. In what respect was the condition of the Dead Sea then different from what it is now? There were two differences and two only; First, the sea, we have strong reason to believe, did not extend to the same extent as at present, its southern part being probably dry land, on which stood the cities of Sodom, Gomorrha, &c. Then, second, the land was not then impregnated with salt, but was fruitful, well watered, and exceedingly fertile, like the land of Egypt, which could not be if it was impregnated with salt. Two changes have taken place, then; the water has risen slightly, and it has become salt. Both of these can be accounted for by one geological mutation, viz., the lifting up of the great salt mountain. There is no doubt that the great salt mountain has been lifted up, since you find surrounding it on all sides a deposit of marl, containing also a large quantity of gypsum, from twenty to sixty feet thick, which deposit is found also at the *top* of the salt mountain. The mountain is from 300 to 400 feet high, and the inference hence is, that its top stood originally at the same level with the surrounding marl, but that the mountain has been pushed up with the deposit on its top. The same thing is stated by Mr. Tristram to be observable in the salt mountains of the Sahara. This elevation of the salt will account for the rise of the water, because when salt is dissolved in water it swells its bulk; and here I do not hesitate to say, that if you could take the salt out of the Dead Sea, you would not only leave the southern lagoon entirely dry, but would also sink the water level of the northern basin several yards deeper. The elevation of the salt mountain is thus quite sufficient to account for all the changes in the Dead Sea district since the days of Abraham. Now what was the state of this ravine before the days of Abraham? What traces are

there of changes still earlier in date? changes, that is, which occurred more than 4,000 years ago. The next point of evidence is, that the whole ravine to a height of some 300 to 400 feet was filled with fresh water. How is that proved? Because you find remains of ancient beaches traceable the whole way round at uniform levels, varying in height from 30 to 200, 300, or even 400 feet. But how can we tell that the water then was fresh water? Because there are shells found in these beaches, and the shells are invariably fresh-water shells, shells of exactly the same species as are found to this day in the Jordan. I was doubtful on this point when reading Mr. Tristram's book, and being then working at the subject of the Dead Sea, I wrote and asked him if he had found any marine shells in these beaches, and he said in reply that there was not a trace of one, they were all recent fresh-water ones. Our next step back is then to show that the Dead Sea was neither a salt sea, nor a small sea, but an enormous fresh-water lake. The fresh-water lake was gradually dried up, not quickly nor uniformly, for it left marked beaches only at intervals, whereas had it dried up quickly, it would have left *débris* all over the shore. Allowing, then, time enough for the formation and slow drying up of the fresh-water lake, what comes before that? We have yet to account for the salt. The only way we can imagine such an enormous mass of rock salt to have been formed—the mountain is about eight miles long, half a mile wide, 300 to 400 feet high, and how deep no one knows—the only reasonable way is to suppose that an arm of the sea was shut in here, dried up, and left the salt. Now, when you have accounted for the rock salt, where are you? Still in the post-tertiary period; not a single geological formation proper has been touched. We started, then, in the days of Abraham, nearly 4,000 years ago, with a small, probably brackish sea, before which was a fresh-water lake, before that an arm of the sea, and still nothing but post-tertiary remains. How much time have we to dispose of for these changes? From Abraham to the Deluge is about 360 years. I ask, then, is it credible, when 4,000 years have done next to nothing, we should suppose that the previous 360 did so much? More than this 360 years we cannot allow, if the current view of the Deluge be true, since if the sea swept across this district at the time of the Deluge, all traces of a preceding fresh-water lake must have been destroyed, and we are thus obliged to suppose that the lake, at all events, was formed and dried up within 360 years of the Deluge. But I am not sure even of all that 360 years, for I have started from the point when I *know* the cities of Sodom, &c., were standing; how long they had been so, I do not know; they may have stood for a considerable part of the 360 years. I say, again, is it credible that such enormous changes should have taken place in so short an interval, when the last 4,000 years have done so little? Beyond the Deluge we have but 1,600 years to the Creation; to which period, therefore, must the whole of the geological formations be referred, if such views as Mr. Hopkins's are to be maintained. I think that is a strong case of what history can tell us as to geological changes, and I cannot but wish simple facts like these were more looked to, before

theories are propounded as to the time probably consumed in such changes taking place.

Rev. WALTER MITCHELL.—It is, perhaps, a pity that some notes for discussion which Mr. Hopkins has sent us were not read previously to Mr. Warington's remarks. I think they contain some facts bearing upon the paper which will meet some of the objections of Mr. Warington. I shall now read them :—

"TERRESTRIAL SUPERFICIAL CHANGES.—The late M. Arago, the French astronomer, in a very elaborate paper brought forward innumerable proofs that the *northern limits* of the growth of the best wine-grapes in France and other places on the Continent were gradually retrograding southward. Many places where, a few centuries ago, grapes of superior quality grew in abundance, are no longer capable of producing ripe grapes."

"Since the discovery of America, and the cultivation of the sugar-cane and tobacco by the Europeans, the northern limits of the growth of these products have very considerably retrograded southward, according to observations made in the United States."

"THE ARCTIC REGION.—*Extracts from the Journals of Arctic Explorers.*—The Gulf Stream renders the sea between Spitzbergen and Nova Zembla comparatively warm and free from ice. The coasts of Spitzbergen contain large quantities of drift-wood from all climates."

"*Bottle-records* conveyed by the Gulf Stream to the Arctic Sea have proved that they never return, but are generally thrown on some of the islands or coasts of the Polar Sea."

"SURVEYS OF LANDS, COASTS, &c., &c.—The configuration of coasts is subject to such changes as to necessitate the employment of a staff of naval officers more or less constantly, to ensure the safety of navigation. The coast of Australia is different from what it was in the time of Cook and Flinders. Even our local trigonometrical surveys are not completed before the first maps require some correction and revision."

I may say here, partially in defence of Mr. Hopkins, that whatever we may have to say with regard to the theory he has brought forward, we cannot but thank him for the immense mass of facts he has adduced—facts of the greatest possible importance in coming to any decision upon such an important question. His theory may be faulty in many respects, but as these motions have been observed for so few years comparatively, (that is, the smaller motions, some of the most important,) we do not know whether they do occur at any regular rate or not, and it would require many years probably of careful observation before that rate is fully determined. I should not altogether, perhaps, myself be inclined to agree with Mr. Hopkins in one portion of his theory, namely, the gradual spiral movement of the earth from the South Pole up to the North Pole, and then the dipping down of the earth through the earth's axis. That is the way in which he accounts for his theory, and I think that escaped Mr. Warington—

Mr. WARINGTON.—I looked for it, and could not see it.

Rev. W. MITCHELL.—He says, "The globe, with all its elements, might have existed from eternity. The ocean and the lands emerging from the Antarctic Pole merge again into the Arctic Pole, and thus circulate from Pole to Pole through the medium of the earth's axis." This he gives as a

purely hypothetical thing, he does not quote his previous facts in proof of this movement, because he says, "This question," that is, such an hypothesis as this, "I conceive to be beyond the reach of demonstrable science," but he pledges himself to the spiral movement of the earth. But a great deal of the crushing Mr. Warington has described, we do find demonstrated wherever we go in northern latitudes. If, for instance, we observe the contorted rock strata of slate, no one can look at such twisted and contorted strata—which at one time have necessarily, from the formation of the strata, been horizontal—and see them crumpled up as you would crumple layers of paper, without being aware there is some terrific force in existence, and doing this somewhere in the earth. None of the popular geological theories give us the slightest theory to account for such crumpling as this, nor the manner in which successive masses of earth are broken and laid one over the other. If I assume the present phase of Geology, and take the popular theory of Sir Charles Lyell that the earth has always been going on as it is now, and that we have merely certain subsidences and upheavals; how are we to account for the great distinction that there is in the successive fauna which present themselves when we take the strata of one layer, and find it covered by another layer and other strata? The other popular theory, scarcely yet gone out of the text-books, was this,—that these fauna belonged to one creation and then they were covered by the fauna belonging to another creation, and that followed by a third creation. And what stopped that hypothesis? Why, the discovery that there was a certain percentage of the fauna of these lower creations intruding upon the upper, and a certain percentage of the fauna of each creation intruding itself upon the other fauna. Now, according to all these old hypotheses, without some power bringing the fauna of one zone over the top of another and a third over that, we want some such theory as Mr. Hopkins supplies, if we are now to believe that all these three fauna were not fauna of distinct creations, but might have been co-existent on the earth at the same time. To take an example from known facts, we find that owing to the course of the Gulf Stream upon one portion of our coast, or of the coast of Europe, we may have an African fauna, and within a few miles of that a northern fauna, brought by the return of the Gulf Stream. I say that according to all our present modes of reading the Palæontological records of the earth, that as regards these places within a few miles of one another, if the mass of earth from one part could be carried and deposited on the earth a few miles north of it, we should have all these phenomena of certain percentages of fauna intruding as it were from certain strata into others, and we should have very much in point of fact what we do find displayed in the various superincumbent strata of the earth. It may be said that this could not have been done within the limits of the time assumed, and that Mr. Hopkins has made a great mistake in his calculations. A want in Mr. Hopkins's paper has been supplied by Mr. Warington's objection. The crumpling up, as it were, of one stratum on another is just what is found to be a fact. It must also be remembered that you could have a motion of the globe moving freely over itself; and that the theory (I think it is Sir Henry James's) of the present solid crust of the

earth, as it were, moving slowly over an envelope, would account for almost all the motions in question. If you put one globe over another, you could, without having this dipping down of the actual earth through the medium of the axis, and circulation from pole to pole, account in a great measure for the changes which are mentioned by Mr. Hopkins, without that great crushing and crumpling which Mr. Warington has pointed out must take place if 10 or 12 miles are squeezed into nothing, owing to the difference of area and surface at the Polar regions. But there would be crushing owing to another cause. There would be no crushing if the earth was a sphere ; but if a prolate or oblate spheroid you would have a great deal of crushing. Now, with regard to what Mr. Warington has stated respecting the Holy Land, I do not think he has made out so strong a case against Mr. Hopkins's hypotheses, as at first sight may seem. The temperature of any place on the earth's surface, I believe, depends upon two things ; it depends upon the height above the level of the sea, as well as the latitude and longitude. It also depends on such causes as the Gulf Stream and proximity to the coast, so that there are many things which modify the effect of latitude. Isothermal lines are very different lines even for places at the same height on the earth's surface ; they are irregular in their character and very different from the lines of latitude, so that we cannot always calculate the heat of a country by merely knowing the latitude and longitude. (Hear, hear.) Again, height has a very considerable influence. We know that in the Himalayas you may, within an hour or two's journey, pass from all the tropical plants up to those almost of Siberian character. The same can be done in the Andes. You may in the morning breakfast amid sugar-canes, in a tropical climate, and in the region of rice. A few hours may transfer you into the region of the grape and wheat, and you may go on until you actually come into the frozen regions. Now I think Mr. Warington has pointed out that there has been not altogether a fixed climate in the Holy Land, but that a considerable change has taken place. Unfortunately the change is not what Mr. Hopkins would have, because it has been a change rather from cold to heat. But there is another thing which might account for that. Has there been any very great depression of this country ? Well, I do not think that in the Biblical times there were any accurate trigonometrical surveys, or astronomical data, but I think Mr. Warington differs slightly from Mr. Tristram in one particular. I think he considers that the greater portion of the Dead Sea has been formed since the days of Abraham, or since the destruction of the cities of the plain—

MR. WARINGTON.—About one-third. But I do not think Mr. Tristram holds that opinion.

REV. W. MITCHELL.—Now, there is something remarkable about the position of the Dead Sea. I suppose it is about the most depressed part of the earth's surface. I think the rapid trigonometrical observations were made and taken under extreme difficulty by Lieutenant Lynch and the American surveying party (but I think their conclusions have been verified and carried out by the survey of our own engineers) ; and what have they

taught us? What fact have they brought to light? The fact that the Valley of the Jordan, at least the greater part of it, and certainly the Dead Sea, is very considerably below the level of the Mediterranean. How many feet is it, Mr. Warington?

MR. WARINGTON.—1,300 feet.

REV. W. MITCHELL.—The level of the Dead Sea is 1,300 feet, as Mr. Warington admits, below the level of the Mediterranean. Now, I do not think that we have any positive record of the existence of this Dead Sea before the time of Abraham. I know some would maintain that the Bible gives no exact statement that the Dead Sea occupies the position of the cities of the plain; but there is a generally received tradition that the Dead Sea owes its formation to the destruction of those cities, and I believe that universal traditions are generally founded upon fact. (Hear, hear.) Now, if the Dead Sea does owe its formation to the overthrow of the cities of the plain, who can tell us at what rate that enormous depression, of 1,300 feet below the level of the Mediterranean, was taking place? It may have been a slow, or it may have been a rapid rate, and it may have been slow enough to account for all these beaches of marl and fresh-water shells, all the products of the Jordan—

MR. WARINGTON.—They are all above the level of the old cities to the extent of 200 feet.

REV. W. MITCHELL.—There I join issue with Mr. Warington, and I say no one knows the site of the old cities—

MR. WARINGTON.—They were in the plain, and not on the mountains.

REV. W. MITCHELL.—What plain? The plain described by Mr. Warington just now as a fertile plain; not a salt or barren plain, but a well-watered plain; a country to be envied; one that Lot chose when he went and resided in the cities of the plain, because it was a fertile country, a goodly country, a country that Abraham allowed his nephew, Lot, to take, as it appeared to be the better portion. But what changes have taken place there since! Whence this withered country—this awful sea, for it is an awful sea? If any one would acquire an idea of the awful character of this sea, let him read Lynch's account of it, who measured its depth—who plumbed it—who was, day by day, exposed to the fierce, burning sun, and to the smarting sensation of the salt vapours, and the sulphurous fumes, and all the other deadly emanations of this sea—who felt that he was in a "cursed land"—who tells you that no one could stay as he stayed there, without feeling that this was the kind of land that you would say, as it were, God's breath had blasted for some fearful crime! I think these are the words of Lynch, or something like them (I do not profess to quote his words accurately), but I know he does say that that is the place to which he would bring the infidel and the scoffer who would doubt the truths of Scripture. But what I want to point out is, that we must have had great changes going on, if these cities of the plain are to be sought underneath the Dead Sea. If that depression is still going on, is there no corresponding depression of the other parts of the country, and might not that be sufficient to account for the change of the climate from cold to heat—viz., being depressed and

coming down to a lower level? I only mention these things to show that we are not, on this ground, at liberty altogether to reject the facts, and many of the deductions that Mr. Hopkins has made from them. There are some other points :—for instance, Mr. Warington has asked, with some degree of triumph, how we are to account for a still greater degree of heat in the Holy Land, if glaciers have cut their way through the ravines of the Lebanon, and marked their course in these ravines? In the first place, I might say that considerable doubt has been felt as to these glacial markings in many places; but, even supposing we admit them, we have one portion of Mr. Hopkins's paper bearing the test of history, for Mr. Warington very properly says we should have history to test these things. Well, history has given us the change of the climate of Greenland. (Hear, hear.) Is it *Greenland* now? Has it any pretension to the name of Greenland? Would any voyager now call it Greenland, or would he not call it White-land, an ice-bound land? But there are other matters to be taken into consideration with regard to this. I am one of those who do believe in a universal deluge; and a universal deluge could not have taken place without a very considerable change of the whole earth's surface, and without leaving very considerable marks on the surface. And the reason, I think, why the popular theorists in geology of the present day do not find the same marks of the deluge that a Cuvier could find, is because they look only for superficial marks, instead of looking for great and gigantic marks. (Hear, hear.) I have heard of another theory of deluges. There is the theory of *Adhammer*; and, though I am not going so far as Adhammer does—namely, to a succession of deluges, one after another—certain I am of this, that Adhammer, both with the acumen of a good geologist, and of a sound physical and mathematical observer, has shown the manner in which a deluge could have taken place—a deluge which would have swept the whole of the newly-formed earth with gigantic masses of ice. I know no other theory which will account, in the slightest degree, for “the glacial period” hypothesis. (Hear, hear.) I know no theory which has ever been propounded to account for the glacial period, which can at all compete with Adhammer's theory—I won't say of deluges, but of one deluge; and, perhaps, the time may come, when science advances far enough, when we shall have patiently accumulated a sufficient number of facts to account for all the paradoxes which we do meet with in the phenomena which geology has given us. We have not only to account for the palm flourishing in this country, and for its having once enjoyed a tropical climate, but we have to account for mountains of ice floating over the country from one end to the other, and I believe that, without extended periods of millions upon millions of years, Mr. Hopkins has, at any rate, sketched out for us a sufficient number of facts to cause us to suspend our judgment before we accept these very great and lengthened periods of time to account for things which may, perchance, (we will only say “may, perchance,”) be included within the limit of some six or seven thousand years, instead of millions and billions of ages.

MR. WARINGTON.—Might I add two words in support of my view, that the Dead Sea stands in the same place as in the days of Abraham? I should

have mentioned it at starting. It is this. We are told in two places in the book of Genesis of Abraham going to look at the Valley of the Jordan : once at Bethel, and again he goes out of Mamre to some other place on the road towards the cities of Sodom, and obtains a view of the cities. These two identical places remain at the present day ; the two places from which the Valley of the Jordan and the site of Sodom can be seen. A man residing in Hebron would have to go to identically the same spot to get the first sight of the Valley of the Cities of the Plain. It seems that is a forcible ground for believing that the level was the same as in the days of Abraham.

REV. WALTER MITCHELL.—I do not see that at all. I admit the fact Mr. Warington states, but I do not draw his conclusions. He does not show that the sea existed before the time of Abraham, nor meet the question I have supposed, of a change of the whole mass of the country, because there might have been a total depression of the country ; but you might have all that depression of the Dead Sea, and yet still Abraham might have looked over the mountains in the same direction, and towards the direction of where the plain sunk. I believe there has been a gradual sinking there, and that alone would account for the change.

CAPTAIN FISHBOURNE.—I observe that Mr. Hopkins does not dwell upon the question of alterations of climate, except as to facts. He merely gives them, as far as they go, to prove other facts, to substantiate other facts. He is equally aware, as Mr. Mitchell, that various circumstances will alter the climate. Mr. Warington admits the fact that there has been an alteration of latitude—the facts he cannot deny. Now, going to Egypt, there is a very distinct alteration with respect to the Pyramids. They have been moved in their position, and astronomical observations distinctly mark a change. But for a still more recent instance, let us go to the other side of the world. In Philadelphia the streets were laid out north, south, east, and west, but they are now changed, though it is only a very short period since the city of Philadelphia was founded. Again, with respect to the sites of churches, the sites of old churches were generally laid east and west, but now they are found to have changed ; and how are these things accounted for ? It is evident there is some cause, some power, which has produced these changes. What is the cause ? It may not be magnetism, as suggested by Mr. Hopkins ; there may be something more. It has been suggested, just as the tides are acted on by the sun and moon, and because the water is mobile and the earth is not, that the tides oscillate backwards and forwards, whereas only portions of the earth oscillate. The whole earth moves, but in proportion and degree as it is mobile, and not all at the same rate. There may be chains of mountains not subject to the same forces, but which do not move until considerable pressure has taken place, and then move by convulsion—for instance, the Cordilleras were moved by one action. Geologists say this is a volcanic operation, but mechanical philosophers say, “No, it is a mechanical operation of the attraction of the sun and moon.” We know the formation of the earth, the diameter at the Equator is twenty-six miles greater than the polar axis. Well, the supposition is, that this mass is in motion, and that may be produced by the

magnetism which Mr. Hopkins supposes is the cause. But magnetism may not be the cause, but the result, of the operation. It is tolerably clear that there has been a motion in the crust of the earth—that is undeniable; and what Mr. Warington has said with respect to Syria is rather the exception to the rule, and may be accounted for just as Mr. Mitchell has been explaining.

MR. REDDIE.—It is somewhat unfortunate, my lord, that Mr. Hopkins, the author of the paper read this evening, has not been able to be present himself to defend it. But I may venture to say this, that I am sure he will be extremely obliged to Mr. Warington for his valuable criticisms. It was never intended that this paper should be accepted here, as absolutely solving the great difficulties that there unquestionably are as regards what is called “the precession of the equinoxes,” whether we endeavour to account for them by the motion of the earth’s axis, or the motion of the whole crust of the earth. I am afraid Captain Fishbourne was assuming the point at issue in taking for granted that the apparent alteration in the position of the streets of Philadelphia, and in the orientation of churches, must be caused solely by the motion of the crust of the earth. It would be equally explained by what astronomers have given as the cause—(at least, if I cannot say *equally* explained, I may say that it would be *approximately* so explained); but then what Mr. Hopkins rests upon, in favour of his view as against the astronomical one, is the existence of those other facts which do appear to afford the proofs of a change of climate having taken place in different parts of the earth, and which Mr. Warington has entirely passed over. Of course it was no part of Mr. Warington’s duty to meet the other side of the case, so to speak; but at the same time, we must not forget that he did only meet one side of it. He did not account for the remains of tropical plants and animals found in Portland and Sheppey, and in the present latitude of London; and he took no notice of the change of climate in Greenland, as Mr. Mitchell has pointed out. Mr. Hopkins, however, will no doubt himself reply to the most important parts of Mr. Warington’s criticisms, especially as regards the exact degree of obliquity of this supposed motion. I believe there has been a slight misunderstanding about it, but nothing that Mr. Hopkins will not either satisfactorily explain, or admit to be unaccounted for. We now come to consider those parts of Mr. Warington’s observations which, as it were, lie within themselves, or the supposed mechanical difficulties of the theory. I scarcely think he has quite established that these difficulties which were to him so great, as to this necessary crumpling and crushing, are any objection to the hypothesis now advanced. Because the obvious result of such crushing would be the raising up of the earth’s crust at one place and its depression at another, and these Mr. Warington will not deny to be geological facts; for even when we go to Palestine he tells us of an upheaval there. Now, Mr. Warington is quite right, that if the earth is being twisted round, and a larger quantity of its solid crust is compressed into a smaller space, there will be this crushing; but what, on the other hand, will there be if the mountains are upheaved by expanding the surface of the globe? Would there not then be a riving asunder, an opening of the earth’s crust, which is not the fact? It appears to

me, these upheavals and depressions *without* forming gaps in the earth's crust are just the very difficulties in geology that Mr. Hopkins's paper tends to solve. Geological authorities now admit that mere upheaval and depression do not account for the phenomena. In Professor Ramsay's inaugural address to the Geological Section of the British Association he says :—

“In the Alps we find areas half as large as an English county, in which a whole series of formations has been turned upside down. But by what means were *masses of strata many thousands of feet thick bent and contorted, and raised into the air*, so as to produce such results, and thus affording matter for the elements to work upon? Not by igneous or other pressure and upheaval from below, for that would *stretch* instead of *crumpling* the strata in the manner in which we find them, in great mountain-chains like the Alps, or in less disturbed groups like those of the Highlands, Wales, and Cumberland, which are only fragments of older mountain-ranges.”

Now, if we regard the earth's crust as a whole, comprising its hills and vales,—and in these vales I especially include the great beds of the ocean,—it does not form one rigid smooth plain ; and even supposing it to be crushed together or compressed into smaller space, if we consider that it is not level, but formed of materials unequal both in their constitution as regards stiffness and pliability, and also in elevation and depression, the result would be that mountains would be raised higher, while at other places there would be depressions, by means of that very compression. I do not say that this would be the result universally, for we must further consider the slowness of this motion, and the waste of solid material that also takes place in various ways ; as, for instance, from the very atmosphere crumbling down even the hardest granite rocks, and from igneous action below ; for although we do not hold, I suppose, now, with the igneous theory that we were taught to believe for a long time, still we know there is burning going on below some parts of the earth, and a certain amount of solid material is thus disposed of. And even this internal heat, it seems, might be the result of this crushing and jamming together that Mr. Warrington finds so difficult to understand. At all events, this subterranean combustion, and the throwing out of materials from below, will make room for the fresh material, to be jammed and crushed together. Of course we know that this paper now puts before a scientific meeting, I think for the first time, a series of views perfectly heretical in geology, and perfectly new, though the facts on which they are based are pretty well known to all ; and in my opinion Mr. Hopkins has put forward his hypothesis to account for them very fairly. He has worked at it for many years, and has endeavoured to gain the ear of the public by means of his very valuable work on *Terrestrial Magnetism and Geology* ; and we know that Professor Kirk, when at our request he was kind enough to give us a review of the whole theories of geology, was driven, to a certain extent, to the acceptance of Mr. Hopkins's views, as affording the best explanation of those facts, which neither the igneous nor the aqueous theories, nor the upheavals and subsidences of other theorists, could properly account for. Now, that being the case, at least it is of great consequence that this theory

should be fully discussed, and certainly of the greatest advantage to it that it should be as severely criticised as possible. No one can have any interest in accepting what will not stand criticism ; and I have no doubt whatever, that as regards anything requiring a reply in what Mr. Warington has advanced, Mr. Hopkins will be prepared with that reply, or if not, he will acknowledge that Mr. Warington has so far refuted his propositions.

The Meeting was then adjourned.

REPLY BY MR. HOPKINS.

I much regret that a severe illness prevented me from attending the meeting to enable me to reply personally to Mr. Warington's remarks on my paper. I trust, however, that the following observations will suffice to clear up some of the obscurities referred to, and also to remove the misapprehensions under which Mr. Warington appears to be labouring. I was somewhat surprised at Mr. Warington's difficulty in commencing his observations. He could not see how a right-angled triangle having an angle of $23\frac{1}{2}^{\circ}$ between the hypotenuse and base, would give a ratio of 50 for the hypotenuse to 20 for the perpendicular. I hope he has since seen that this is correct, as it can be easily proved to be so, and I consider it therefore unnecessary to take further notice of it. In discussing the arguments brought forward in the paper, to prove that the surface of the globe has a motion which shifts the lands from south to north, it is necessary that we should keep our minds entirely free from all preconceived ideas, and restrict our thoughts to the observed conditions or ascertained facts. Now, in examining the lands, we have discovered two primary facts:—1stly, that the lands are subject to constant changes ; and 2ndly, astronomical observations have proved that there is an annual change in a given direction equal to $50''$. During the last 2,140 years Alexandria has moved 30° towards the N.W., and has advanced in the same time 12° in latitude north. This is the total amount of the movement founded on actual observations. As this movement is found to be constantly going on at the same rate and the same direction, we may naturally conclude that it has been going on since the days of the creation. Such a superficial movement cannot take place without changing the latitudes and the climates of the shifting lands. Mr. Warington admits the fact "that the latitude had altered to the extent stated ;" but he endeavours to maintain "that the alteration of latitude does not involve a change of climate." Mr. Warington remarks, "Granted that the latitudes of Greenland, of England, of Australia, have varied to the extent that he (Mr. Hopkins) says they have, and I am quite prepared to admit it ; still this does not involve the change of climate. The latitude has moved, but the climate, we have no ground for

thinking, was thereby altered in the least." Such an opinion is certainly extraordinary, as it is well known that the climate of countries depends more or less at corresponding elevations on the latitudes, and we have most incontrovertible evidence of the changes which have taken place in Greenland and England as well as in other places. The Icelandic chronicles not only refer to former productions of that island, to forests of birch and fir, and the cultivation of barley and other grain, but also to the forests and the inhabitants of Greenland prior to the Norwegian emigration, and mention the name of a warm and fertile valley (Kirkinbui) near the southern coast. About 1,400 years ago there was a country called Vinland, within a few days' sail of Greenland, watered with rivers yielding abundance of fine salmon, on the banks of which were trees loaded with agreeable fruits, the temperature pleasant, and the soil fertile. Large stumps of the trees of the forests are still seen in Greenland.

The monasteries in the south of Denmark, in the thirteenth century, were confirmed by the papal rescripts in their possession of vineyards. Various documents of the 12th and 13th centuries testify that the wine-grape was grown at that time in the south of England, as was also the case in the north-west part of France (Brittany and Normandy), where it is not cultivated now any more than in England. The climate has become colder, and in this way the vineyards of north-western France and England have vanished, the limit of the vine being driven further south. In the east of Germany the vine-limit was further north formerly, beyond the districts which are now in the parallel of 53°. Mr. Warington does not appear to be aware of these changes, and he ignores the geological facts altogether. He refers to Palestine, and attempts to sustain his views by reference to the botany and zoology of that country, which he positively maintains are now the same as they were of yore. Let us test the correctness of his arguments by the records :—

Mr. Warington asks, "Does Biblical evidence show us, that in the days of Moses Palestine was in the tropics?" "The vegetation now observable in Palestine is identical with the vegetation mentioned in the Pentateuch. You have the oak, the terebinth, &c., as the characteristic trees then, just as now; the palm mentioned but seldom, and as found only in certain places, as in the valley of the Jordan, just as at present. In the same way, also, with regard to the zoology of Palestine, we know perfectly well that the plants and animals, the zoology, and botany of the country at the present day are exactly those which the Bible describes." I shall now endeavour to satisfy Mr. Warington on these points, and would draw his attention to the accounts of former tropical productions in Palestine, such as groves of palm-trees and cedar-trees, as well as the balsam; also to the lions, leopards, &c., referred to in the Scriptures. Before, however, I enter into the question connected with the botany and the zoology of the country, I think it necessary to give a general idea of the configuration of the surface. The physical character of Palestine, like that of Ceylon, renders it capable of producing and nourishing all the organic productions of the world. In no other districts of similar

size, with the exception of Ceylon and some parts of the Andes, could the typical flora and fauna of so many distinct regions and zones be brought into such close juxta-position as in Palestine. It contains four regions distinguished by difference of climate, and necessarily different productions. 1st. In the lowest depression along the valley of the Jordan the temperature is from 70° to 80° . 2ndly. On the plains 500 feet high the temperature varies from 65° to 70° . 3rdly. On the table-lands, from 2,000 to 3,000 feet high, the temperature is from 55° to 63° . 4thly. On the mountains of the Lebanon, from 4,000 to 10,000 feet high, the average temperature is about 35° . Hence it will be observed the country must have been capable of producing all the productions of the world, from the tropics to the Alpine regions. The Lebanon ranges are never free from snow. These mountains were over-shadowed with fir and oak trees, and in the valleys below grew magnificent cedars, the latter being tropical trees. The ostrich approached the southern borders. Animals of different climes met in Palestine; but the lions, leopards and panthers have long since disappeared. The lion is a tropical animal. Formerly lions infested Samaria, and frequently attacked the inhabitants. Mr. Warington refers to the killing of a lion in the snow, but seems to forget that the lion was a tropical animal. The palm-tree is a very characteristic tropical plant, and is much esteemed for its various productions. Now, the palm-tree and the balsam-tree were two peculiar trees of Judea. The groves of palms were tall and beautiful, and abounded in Judea. Jericho was also celebrated for its palm groves, so that it was termed "the city of palm-trees." Even Bethany was called "the house of dates." At the time of our Saviour there were palm-trees near Jerusalem, as we are told in the Gospel of St. John: "The people took branches of palms, and went forth to meet him." Such a rich display of palm-trees is only seen under a tropical sun. The palm groves have long disappeared from Palestine. The vineyards of Palestine at the present time are not very remarkable for their products. In the days of Moses the vines in the valleys were very prolific, producing several crops of ripe grapes during the year. These continuous crops can only be obtained under a tropical sun. "Be ye of good courage, and bring of the fruit of the land. Now the time was the time of the *first* ripe grapes. And they came into the brook of Eschol, and cut down from thence a branch with one cluster of grapes, and they bore it between two upon a staff." "As to the ripe fruit, let them carry that which is *ripe first of all* into the temple." In the tropics several crops of ripe grapes are obtained from the same vine within the year. The country bordering the lake of Gennesareth was formerly very remarkable for its varied productions. Josephus states "there are palm-trees also, which grow best in hot air; fig-trees also and olives grow near them. One may call this place the ambition of nature: it is a happy contention of the seasons, as if every one of these plants laid claim to this country. It not only nourishes different sorts of autumnal fruit beyond men's expectation, but preserves them a great while; it supplies men with the principal fruits, with grapes and figs *continually during ten months of the year*, and

the rest of the fruits as they become ripe together, through the whole of the year." Such perpetual productiveness almost throughout the year is only obtainable under a tropical sun—never in temperate zones. Within the tropics *blossoms* and *ripe fruits* are commonly seen on the same trees, and are very conspicuous on orange and lemon trees. Galilee, Samaria, and Judea had a rich and fruitful soil, full of trees of all sorts—the olive, the vine, and the palm-tree. "They have abundance of fruit trees, and are full of fruit, both that which grows wild, and that which is the effect of cultivation." "Take," said Jacob, "of the best fruits in the land and carry down a present, a little balm, and spices, with myrrh, nuts, and almonds." Where but within the tropics could we see these productions and such a prolific scene in the open air? "For the land, whither thou goest, is not as the land of Egypt, from whence ye came out, where thou sowedst thy seed, and wateredst it with thy foot as a garden of herbs: but the land, whither ye go to possess it, is a land of hills and valleys, and drinketh water of the rain of heaven: a land which the Lord thy God careth for." "I will give you the rain of your land in his due season, the first rain and the latter rain, that thou mayest gather in thy corn, and thy wine, and thine oil. And I will send grass in thy fields for thy cattle, that thou mayest eat and be full." "It is a land that floweth with milk and honey."—Deut. xi. The two wet seasons are peculiarly tropical. In the temperate zones we have summer and winter; in the tropics, wet and dry seasons. The former climate of Palestine must have been somewhat analogous to that of the southern part of Arabia bordering the Red Sea, such as we now find it at Medina, Mecca, and at Aden, where sweet spices, balm, and myrrh still grow. In the days of Alexander the Great, frankincense and myrrh were produced near Gaza. Mr. Warington asserts that "wheat and olive will *not grow in the tropics*." I beg to state that he has been misled as regards these productions, as well as on various other points. I have grown in the very tropical country which Humboldt visited, and within 5° of the equator, grapes, pomegranates, figs, olives, oranges, coffee, pine-apples, corn, &c. I have seen the same variety of productions in Ceylon, Penang, Singapore, &c. In New Granada, on the plains of Bogota, within 4° of the equator, wheat and barley are cultivated in large quantities. Wheat can be produced at 3,000 feet high. In Egypt it is grown at a low elevation during the winter, though not in the hot weather. I hope Mr. Warington will excuse me from referring to the arguments founded on what may be gathered from "Johnson's Physical Atlas," as they must have originated from a misconception, in connection with the configuration of the surface of the earth, and the general data intended to explain why certain products are more cultivated for commercial purposes in some places than in others in similar climates. It may be thought strange that not only wheat, but *flax*, should thrive in the hot and tropical part of Egypt at low elevations, as well as in the cold regions of Russia as far as 64° lat. N. In Egypt flax (as well as wheat) is sown in December, in the fields just quitted by the waters of the Nile, and it is harvested in April before the hot weather sets in. In Russia, it is sown in

April, and harvested in September. The mountains of Ararat are now situated in latitude of about 40° , and are more or less covered with perpetual snow. At the time of the Flood they were in latitude 16° , in a warmer climate and suitable to receive Noah's live stock to replenish the earth—the tropical as well as other animals ; and fit for the subsequent growth of the vineyards which supplied Noah and his family with wine. The country in which Nineveh, as well as Babylon, was situated had formerly its palm-trees, as delineated in the ancient carved marbles. It also had its wild beasts—lions, leopards, &c. ; thus indicating that all that region at the time of the Flood was within the tropics. I was very much struck with the general aspect of the country, from the Nile to Arabia, when I first saw it. The scene presented nothing but an interminable, parched, barren desert, with clouds of sand, from Cairo to Suez. The mountains of Horeb and Sinai appeared as burnt ferruginous rocks without a blade of grass to be seen anywhere—a scene of complete barrenness and desolation. It looked as if it was a land first risen from the deep, as it had but a few patches of marine deposits, from the Nile to Palestine and Arabia, with the exception of some calcareous beds. In almost all other parts of the world in both hemispheres the lands are more or less covered with various sedimentary deposits, and many of these are comparatively recent, as if they had been subject to many undulations, rising and sinking from the level of the sea ; but here, in Egypt, Syria, and Arabia, there are no such indications. This part of the world, after having first risen from the deep to receive and sustain the primeval plants and animals at the creation, remained apparently above water until it was overwhelmed and scoured by the flood. Other parts of the world subsequently rose from the deep preparatory to the dispersion of the human race, and the old primeval antediluvian land reappeared as a rocky, barren waste. The upheaval and subsidence of the level of the Red Sea to Syria do not appear to have been very great during the historical period. The coast from Tyre to Sidon, on the shores of the Mediterranean, has risen several feet. The changes in the South at Suez and the eastern arm of the Red Sea have been principally produced by blown sands and gravel from the desert. At the time of Herodotus the Red Sea extended to Heliopolis. The ruins of that city are now situated inland half way between Suez and the Mediterranean. Suez in 1541 received into its harbour the fleet of Solymán II., but it is now changed into a sandbank, and the passage further north has been filled up with sand blown from the desert. On the opposite Arabian side many of the inland ancient towns (now in ruins) were, since the Christian era, on the sea shore. The blown sand and the rapid growth of the corals have encroached on the sea. The eastern valley, between the Dead Sea and the Red Sea, in like manner, has been gradually filling up with sand.

At the time of Herodotus, the sun in summer—that is, in the month of June—passed over the Mediterranean, and retired in winter to Libya (or Central Africa). “During the winter months,” he says, “the sun, passing over the upper parts of Libya, produces the following effect :—As the air in these regions is always serene, and the soil is always hot, he produces the

same effect as he usually does in the summer, when he passes through the middle of the firmament [that is, the zenith of the historian]; for he attracts the water [the Mediterranean] to himself, and, having attracted it, throws it back again [? as rain] upon the higher regions of Libya." Anciently, when the Delta was within the boundary of the tropics, the hippopotamus was found in the Lower Nile, where he was hunted. Now, this animal is rarely seen, even in Lower Nubia, within the northern limits of the tropics. Large crocodiles were common in the Nile, but now we only see occasionally a few small ones even in Cairo. We have to ascend to the cataracts within the tropics before we meet with large crocodiles.

Hipparchus was the most eminent of the ancient Greek astronomers. After studying at Alexandria, he continued his astronomical observations at Rhodes 34 years. He first discovered the phenomenon called the "precession of the equinoxes;" he catalogued the fixed stars, and laid the foundation for a correct system of astronomical computations. At that period, the northern limit of the tropic extended to Rhodes: hence "the sun in June passed through the middle of the firmament." Ptolemy's physical system of astronomy was introduced by Hipparchus. To show how little the theories of astronomy have to do with the astronomical computations, on which alone the science rests, he was able to calculate the period of the eclipses, the mean period of the planets' revolutions, and, in fact, all the observed celestial phenomena. The same was subsequently done by Tycho Brahe, who also entertained a geocentric theory, as accurately as the instruments then provided could admit. We can have no assistance from the modern theory of physical astronomy to guide us in our discussions, as its very foundation has been completely destroyed, not only by restoring the plenum, or a resisting medium, but also by other new ideas regarding the sun, meteors, &c., &c. It must be borne in mind that the only science of astronomy we can depend on, is that founded upon a *system of computation*, and nothing else. I was desirous to restrict my paper on this occasion to giving a brief description of the observed facts connected with the movements of the surface of the earth from south to north. I thought that, as the operations going on at the poles could not be examined, and therefore would have to be determined from analogy, they might be left for future consideration. But whatever may be the opinions as to the character of the actions going on at the poles, they cannot affect the fact of the great superficial movement of the earth from south to north. If we look at the maps of the southern and northern hemispheres, with their respective poles in the centre, it will be seen that the dry lands radiating from the Antarctic Circle are comparatively very limited, and they are composed principally of gravel and sand; whereas, in the northern hemisphere, they are crowded, jammed, contorted, rising in ridges, and they contract the passage of the currents of the ocean to the north polar basin. A superficial glance will show that the dry lands, after passing the parallel of 40° N. lat., become so crowded as to allow but a small space for the ocean, as compared with the other parts of the globe.

The Pacific passage (Behring's Strait) is becoming very narrow. The northerly action of the lands, and the contraction of the space as they approach the Arctic Circle, have apparently caused a lateral pressure, which has squeezed the sedimentary beds of the United States into a series of narrow troughs, which represent the coal-seams in the transverse section in the form of UUU's, thus reducing the space formerly occupied by the beds to about one-fourth the width. The same effect is seen in Europe in the coal-beds of Belgium and Prussia. The beds of the coal-measures are squeezed so much as to represent in the sections very acute angles, and in several places the seams are actually seen in a perpendicular position. The seams of the sections in general appear like very acute WWW's. The original space, in many cases, has been reduced to from one-sixth to one-eighth. As regards the vertical primary rocks below, they can be well observed in the north coast of Ireland and Scotland; also in Norway. The silicious bands become more compact and contracted, whilst the talcose, micaceous, and argillaceous bands are squeezed out and produce polished striae, which, on exposure, are often mistaken for the effects of the action of glaciers. There is also a rapid disintegration and decomposition of rocks going on as they approach the Arctic Circle, and the margin of the polar basin.

With regard to the character of the terrestrial axis, whose ends are the points of emergence and convergence of all the circulating fluids of the globe, we cannot decide. We can only obtain approximate dimensions of the area of the ends by means of observations founded upon the angle of the dipping-needles and the diameter of the cone of the Aurora. According to such observations, the active polar axis is about 20° in diameter. No Arctic explorer has been able yet to reach it, and actually to observe the operations going on there. We may, however, venture to assume that it is acting like a magnetic axis, as the perpetual circulation of the magnetic currents from pole to pole could not have continued without the action of such an axis. As the globe is a semi-aqueous body, and not a mere ferruginous magnetic shell, it is evident that it is not merely a magnetic globe, but subject to a great electro-magnetic action; and, if so, the axis would not only be the mere conductor of the returning magnetic currents, but the core of the terrestrial battery; the ocean and all terrestrial substances would be decomposed at the North Pole, and be reproduced again at the South Pole in the same manner as by means of an electro-magnetic apparatus in a decomposing trough. This seems to be more probable than what has been suggested by some philosophers, namely, that the axis might not only be porous, but tubular, and thus would admit of the passage of all the substances in solution from one end to the other.

Although we cannot approach the south polar basin, we know that the sea comes from that pole in strong and continuous streams, as from a great fountain, acting by impulses, and thus causing tidal waves. Many attempts have been made to account for the return of the Gulf Stream, or the ocean from the Arctic basin back to the south, and until very recently it was sup-

posed that there was an actual returning current detected in the bottom of the ocean. All these ideas have, however, been completely set aside by the careful soundings made in connection with the Atlantic Telegraph. There are no such currents. The bottom of the sea is comparatively quiescent. Therefore it is quite evident that the oceanic streams, as they arrive at the north polar basin, and are absorbed therein, are either decomposed, as in a battery, or are made to pass through the axis and to re-appear again at the opposite end. We know from daily experience in subterranean operations in the primary rocks, that the electro-magnetic currents of the earth are very powerful and active, and are constantly reproducing and decomposing the various minerals of which the earth is composed. Therefore, since this is the fact, we may reasonably conclude that the same kind of action must go on at the poles ; and when we consider that the entire electro-magnetic force of the earth is concentrated and converged at the north polar basin with a saline liquid to act upon, over an area of 20° in diameter, it must have sufficient power to dissolve all the substances of the earth as rapidly as they arrive there, and to reduce them to their primary elements, making them to re-appear at the same rate at the opposite pole, atom for atom, or crystal for crystal.

NOTE.—Owing to the lamented death of Mr. Hopkins at the time when his Reply was in the printer's hands, it has not had the benefit of his own final revision ; and the Editor must therefore be held responsible for any inaccuracies or imperfections which may appear therein.

ORDINARY MEETING, MARCH 4, 1867.

THE REV. WALTER MITCHELL, VICE-PRESIDENT, IN THE CHAIR.

The minutes of the previous Meeting were read and confirmed.

The HONORARY SECRETARY announced the names of the following new members and associates.

MEMBERS :— John Griffith, Esq., 6, Hanover Terrace, Regent's Park ; W. Castle Smith, Esq., F.R.G.S., M.R.I., 1, Gloucester Terrace, Regent's Park (*Life Members*).

The Hon. Charles Barter, B.C.L. Oxon. (Member of the Legislative Council of Natal), Pietermaritzburg ; the Rev. D. A. Beaufort, M.A., Warburton Rectory, Warrington ; George Pigot Moodie, Esq., Gov. Surv., J. P., Melsetter, Pietermaritzburg, Natal ; P. C. Sutherland, Esq., M.D., M.R.C.S. Edinburgh, F.R.G.S., Surv. Gen. Pietermaritzburg, Natal.

The above Members are on the Foundation List.

MEMBERS for 1867 :—R. G. M. Browne, Esq., 9, College Crescent, Hampstead ; Thomas Lomas, Esq., H.M. Civil Service, Alma Villas, Windsor, Berks.

ASSOCIATES, 1ST CLASS :—W. F. Browell, Esq., Barrister-at-Law, J.P. for Kent, Broadlands, Tunbridge Wells ; 2ND CLASS :—The Rev. Philip Dwyer, A.B., T.C.D., Vicar of Drumcliffe, Bindon Street, Ennis, Ireland ; the Rev. Walter S. Grindle, Theol. Assoc. K.C.L., 26, Bessborough Street, Pimlico W. R. Warwick, Esq., M.D., Southend, Essex.

MR. WARINGTON then read the following Paper :—

ON THE CREDIBILITY OF DARWINISM.—By GEORGE WARINGTON, Esq., F.C.S., *Mem. Vict. Inst.*

IT is a rare circumstance for the full explanation of any phenomenon, or series of phenomena, in nature, to be discovered at once. In respect to the most certain, as well as the most uncertain of the interpretations of Science, there has been in nearly every case a period of speculation, of theorizing, in which the view ultimately accepted as true was merely an hypothesis. From the very nature of things it must be so. A certain interpretation is not to be arrived at without a widely-extended series of facts on which it may be based,—facts often requiring long and laborious investigation to accumulate. In such accumulation, carried on with the express purpose of obtaining an explanation, it is impossible but that various hypothetical explanations should suggest themselves to the inquirer, one of which will be almost certain to approve itself to his mind as the most probable. This immediately becomes his theory; to ascertain the truth or falsity of which is henceforth his object. It may be that further investigation disproves it, and it is cast aside; only, however, to be replaced by another, which, so far, stands the test of facts. Or it may be that fuller knowledge merely adds strength and solidity to that first adopted. But in either case it is through hypothesis that truth is ultimately attained. Theoretically, of course, the scientific method is first to obtain a full view of all the facts, and then deduce the explanation. Rarely, if ever, however, is this theory carried out in practice. Nor, indeed, can it be; since how, before any idea of the explanation exists, is it to be known *what* facts especially need to be accumulated and sought after? All that the severest Science can demand is that the result, when offered for acceptance as true, shall be capable of being cast into this theoretical mould; the facts when duly weighed and classified being shown exactly and inevitably to imply the explanation given. But that this should have been the actual course of the investigation—that Science has nothing whatever to do with. In a word, to use Darwinian language, the process by which true explanations are obtained in Science is very much one of Natural Selection. Many hypotheses spring up and struggle together for existence; passing on from hand to hand, they become varied and modified; each variation tending to produce harmony with the conditions of life (*i.e.* the facts of the case) favours prolonged existence; each

variation tending the other way leads to extinction; and so at last, hypothesis after hypothesis dying out, that one is left alone as victor which is found to be most perfectly adapted to the exigencies of the case.

I have been led to these remarks upon the relation of hypotheses to scientific conclusions, 1st, because it is such an intermediate hypothesis which we have this evening to consider; and, 2nd, because it is sometimes said that to spend time in estimating the credibility of an unproved theory is unscientific, we should rather devote ourselves to the investigation of facts. But if the principles laid down above be correct, and it is practically impossible to conduct investigation without hypotheses, then it is clearly a matter of grave importance *what* hypotheses we thus tentatively adopt,—a matter which should of course be determined by the amount of credibility at present belonging to them. Some facts, some arguments, some analogies, bearing upon Darwinism, we have already, albeit confessedly insufficient to demonstrate its truth. The question is, then, do these facts, arguments and analogies afford such an amount of evidence in its favour as to render it a fair working hypothesis for future research? Is Darwinism, in a word, credible? Not, is it the true explanation of the phenomena it seeks to account for; but, is it such an hypothesis as *may* possibly in the end prove to be so? If so—if Darwinism be credible, then it ought to be carefully kept in mind, applied, and tested, in all investigations into the facts which it concerns, that so its truth or falsity may become apparent. If, on the other hand, Darwinism be incredible, then it may be at once rejected as unnecessary to be considered, at all events until fresh evidence in its favour is adduced. This is the practical issue which the present paper proposes to raise.

The tests to which scientific hypotheses are subject in the process of selection by which they pass into certain interpretations, are fourfold,—possibility, adequacy, consistency, and harmony. The precise meaning to be attached to these terms, and the value to be set upon the tests they denote, may be best seen by a simple example. Let us take for this purpose the hypothesis that gravitation is the sole controlling force by which the motions of the planets in the solar system are regulated. To test the credibility of this hypothesis we should have to inquire,—1st, Is gravitation a *real* cause, capable by its action of controlling planetary motion; *i. e.*, is the hypothesis possible? 2nd, Is gravitation a *sufficient* cause to account for all the motions actually observed; *i. e.*, is the hypothesis adequate? 3rd, Are *all* the effects in fact produced which

gravitation must produce if really at work; *i. e.*, is the hypothesis consistent? 4th, Is there any evidence of gravitation being actually at work in any part of the solar system, or other similar sphere, which would afford ground of *analogy* for regarding it as probably at work throughout the whole; *i. e.*, is the hypothesis harmonious? Before any hypothesis can be admitted as certainly true, it must satisfy all these four requirements. Until it does so, it can only be accounted as more or less credible; provided always that it answers the first demand—that it is possible. If this be wanting—if there be no evidence that the cause assumed is a real cause, then the hypothesis is purely fanciful and unworthy of credence. But if it be possible, then so far as it fulfils the other three conditions it is also credible. The degree to which any hypothesis fulfils these conditions will depend primarily of course upon its truth; it will also, however, be affected very seriously by the inevitable limitations of human knowledge. It is quite possible for a true explanation to appear inadequate or inconsistent, simply because of our ignorance. Thus, to take an illustration bearing upon the example just reviewed, the adequacy of gravitation, prior to the discovery of Neptune, appeared at fault, perturbations being observed in the planetary motion, for which gravitation failed to account. The discovery of Neptune, which removed this objection, depended, however, on a property altogether independent of its gravitating influence, the property, namely, of reflecting light. Had Neptune been so constituted as not to reflect (which is perfectly possible), no telescope could have descried it, and gravitation might very likely in consequence have been rejected by some as an inadequate hypothesis, when, in truth, the apparent inadequacy arose entirely from the imperfection of our knowledge. It is plain, then, that no objection to an hypothesis should be regarded as of final weight, for which a possible explanation can be given, not inconsistent with observed facts. Weaken the credibility of the hypothesis such objections can and do, destroy it altogether they cannot. On these principles, then, it is proposed now briefly to discuss “the credibility of Darwinism.”

1. Its possibility. Are the elements involved in Mr. Darwin's hypothesis real elements, and are they capable of producing the *kind* of effects he ascribes to them? The elements involved are four:—(1) “Growth with reproduction; (2) Inheritance which is almost implied by reproduction; (3) Variability from the indirect and direct action of the external conditions of life, and from use and disuse; (4) A ratio of increase so high as to lead to a struggle for life.”

The result being "Natural selection, entailing divergence of character and the extinction of less improved forms."* Is it possible for these elements, in their mutual action and reaction, to occasion specific differences in living beings? This is our first inquiry; for the solution of which it is manifestly necessary that we should understand clearly what is meant by specific differences—what is meant by a species. We may define it roughly by saying that a species is a race of living beings differing in certain respects from all other races, such differences being steadily transmissible by reproduction, and not being the immediate result of present outward conditions. By this definition are excluded—(1) all mere transient sports, and (2) all apparent varieties dependent directly upon situation, climate, &c. To many it will doubtless seem far too lax a definition, as raising a large number of so-called varieties into the rank of species. Let such afford a better one, equally sufficient and equally free from arbitrary assertion. The great thing we have to beware of is allowing prejudice to lay down a definition which shall beg the question at issue, *e. g.*, that specific differences are those which are permanently invariable; that species are those races which are not bound together with intermediate varieties, or which cannot be traced to a common origin, or which, when crossed, yield sterile hybrids. To discuss the natural origin of species with such definitions as the basis would be as impossible and absurd as to discuss the motions of the fixed stars with the definition given that the fixed stars are those which never move. The only fair definition of a species is a race of living beings possessing common characteristic differences from all others, which differences at the present time are constant and inherent.

That species grow and reproduce, that they pass on their characteristics by inheritance, and that they are liable to variation, is admitted by every one. The point at issue is whether they can *so* pass on and accumulate their variations by inheritance as in the end to bring about specific differences. If they can do so, then the Darwinian hypothesis of the origin of species is, so far, possible; variation and inheritance *could* bring about specific differences.

Our attention must in the first place be directed to the formation of breeds among domesticated animals and cultivated plants. It is notorious that there have been produced by the agency of man distinct races of living beings, having characteristic differences from all others, which differences

* "Origin of Species," 4th edit., p. 577.

are at the present time constant and inherent. He has begun with a single race, and out of that single race he has produced many, differing alike from their common progenitor, and from each other. These new races breed true, *i. e.*, steadily transmit their peculiarities by reproduction; they are independent of local and temporary circumstances; in fact, they are neither more nor less than species, and would unhesitatingly be recognized as such by naturalists *if* their origin were only unknown. It is unnecessary to instance particular examples, the facts are patent to every one, whether in respect to cattle, horses, dogs, fowls, vegetables, or flowers. How then has man done this? He has done it simply by availing himself of observed natural variations, which he has trusted to inheritance to perpetuate. Directly to produce variation is entirely beyond his power, he knows nothing of its causes, and can in no way influence it. He simply selects, and so controls. The variations in character in individuals of any species do not as a general rule tend to effect any specific change, if intercrossing be freely permitted, because they are perpetually neutralizing one another. But man, perceiving some variation useful to himself, isolates and preserves it by preventing the intercrossing which is calculated to destroy it. The process is repeated generation after generation, with the like precaution, until at last the variation is fixed, it has become specific. While, therefore, these facts concerning breeds prove conclusively that variation and inheritance can produce species, they show, further, that to do this a certain selection is necessary to prevent the counteracting influence of intercrosses. In the case of domestic breeds this is done by man's arbitrary isolation. Is there anything in Nature corresponding to this, and capable of producing the like effect? Undoubtedly there is. In some cases there is the very same thing at work,—isolation; a few individuals of a species are often separated locally from all others, and exposed, therefore, to but little intercrossing. If variations occurred here, there would manifestly be far greater chance, so far, of their being perpetuated and becoming specific, than in a locality where a large number were to be found together. More important, however, than this, as more generally applicable, and really more potent, is the principle which Mr. Darwin has denominated Natural Selection, and which forms the key to his whole hypothesis. All living beings reproduce themselves in a geometrical ratio of increase, which must inevitably lead to an overcrowding, a jostling, a struggle, both for position and subsistence. The fact that it is so is indisputable. What follows, then? Clearly there must be a selection perpetually

going on. Not every seed that ripens can possibly germinate, not all that germinate can grow up, not every one that grows up can come to maturity and reproduce itself; and so in like manner with animals. There is a perpetual struggle for existence going on, both among rival races and rival individuals; this struggle must lead to selection. But selection on what principle? A mere indiscriminate selection would have as little tendency to bring about specific differences in nature as an indiscriminate isolation of individuals would have to produce an improvement in breeds. The selection to be effective must be one which lays hold of particular variations, and tends to perpetuate them, to the exclusion of others. Is this the case here? Again we may say, from the very nature of things it must be so. The selection being mainly of the nature of a competition, it follows that just those races, those individuals, will be successful which are most perfectly adapted to the conditions under which the struggle is carried on. But the variations occurring in individuals cannot but be in many cases of considerable moment to such adaptation, either beneficially or otherwise. If the former, those individuals will be precisely such as natural selection will inevitably tend to preserve; if the latter, they will be such as natural selection will inevitably reject. The same will take place with the descendants of the favoured few, and so by a continual sifting out of those which lack the advantageous variation, or possess it in a smaller measure, the predominance of the altered form becomes yearly greater and greater, the counteracting influence of intercrossing as a consequence less and less, the variation is strengthened and rendered constant, and a specific difference is the result. Granted that species vary, that their variations frequently have a bearing on their adaptation to the circumstances of their life, that they have a tendency to transmit variations by inheritance, that there is a continual process of selection among individuals going on, which of necessity favours those possessed of advantageous variations to the exclusion of others, and there is no alternative left but to conclude that the Darwinian hypothesis is *possible*. The elements contained in it are real elements, their action and reaction exactly that which is asserted; the result is inevitable. The causes assigned by Mr. Darwin for the existence of specific differences, are not only real causes, such as *may* account for phenomena similar to those sought to be explained; but are, further, causes actually at work in the domain where these phenomena occur. Not only, therefore, is the hypothesis possible, but it is also established as to some extent true. Few, if any, probably will deny that there are *some* races of living beings whose

specific differences have been occasioned by such causes as those alleged by Mr. Darwin. This, however, is very far from satisfying the hypothesis, which is, not that some races have thus originated, but that *all* have. Mr. Darwin believes "that animals have descended from at most only four or five progenitors, and plants from an equal or lesser number."* Analogy, indeed, would lead him "one step farther," namely, to the belief that "all animals and plants have descended from some one prototype." But this he regards as "immaterial whether or not it be accepted,"† inasmuch as no distinct evidence can here be alleged. Confining ourselves, then, to the hypothesis proper, the descent of all living beings from some eight or ten progenitors, we proceed to our second inquiry.

2. Its adequacy. Are the causes alleged *sufficient* to account for all the specific differences known to exist? Here it is not pretended that more than an approximate answer can be given. It is not always possible to give even a probable account of *how* certain differences might thus have been occasioned. All that Mr. Darwin asserts is that his hypothesis can be shown to be adequate in so many, even of the apparently most difficult cases, that there is no valid reason on this ground for rejecting it, but rather much reason for regarding it as probably true. To estimate the validity of this position, it will be necessary to examine somewhat more in detail the extent and power of the two great elements in the hypothesis above defined—inherited variation and natural selection. The necessarily limited space of the present paper will render it, however, imperative in doing this to confine ourselves to illustrations of the *kind* of differences capable of being thus produced, instead of fully discussing any one or more crucial cases.

That differences in size, in colour, in detailed form, and the relative development of different parts, occur in species, and are liable to be inherited, no one probably will deny. I restrict myself therefore to variations going beyond these.

And firstly, be it noticed such variations include many striking structural changes. Thus we have such cases on record as of a woman being born with two or three toes of the right foot completely joined together with skin, partially webbed in fact; her children being free from the peculiarity, its reappearance however in some of her grandchildren, now in the foot, and now in the hand, but always on the right side; its perpetuation to her great-grandchildren in like

* P. 570.

† P. 571.

fashion ; and so on for yet another generation. Or, again, of the absence of nails, accompanied with perfect baldness, carried down through four generations ; or of hare-lip, carried down through five generations. Or, again, of deaf-dumbness, transmitted through four generations ; of albinism and other alterations in the eye, similarly hereditary. Especially do such instances prove the wonderful power of the principle of inheritance. At every successive reproduction, the influence of the original variant diminished by one half, so that by the fourth generation it amounted only to one thirty-second, by the fifth generation only to one sixty-fourth part of the total influence. Yet so strong is the tendency to reproduce variations, even when, as in these examples, of a highly disadvantageous or even abortive character, that, notwithstanding, the peculiarity still made its appearance. In a similar way the hereditary character of structural diseases, as consumption, mania, &c., is acknowledged by all. These, then, are cases where we may say everything was against the inheritance of the variation, and yet it was inherited. Had the variations been beneficial, and so themselves have tended to preservation—had, for example, the palmation of the toes occurred in a bird living partly in the water, or the baldness in another to whom head-feathers were inconvenient (and the like phenomenon has been observed to be hereditary in doves); or, again, had similar changes taken place, only in an opposite direction,—say the strengthening of the lungs instead of their weakening, or the addition of pigment to eyes formerly devoid of it, instead of its withdrawal from eyes formerly possessed of it ; had especially, owing to the favourable influence of such variations, and the consequent multiplication of their possession, some of the successive generations been born of parents both of whom varied in the same manner ;—had this been so, we cannot doubt but that races of living beings would have come into existence differing most markedly in structure from their progenitors, and forming species which the anti-Darwinian naturalist would ridicule the idea of ever having sprung from the source they did.

Then, in the next place, it must be observed that such variations extend also to notable differences connected with habit and manner of growth. Thus no one will dispute the marked physiological distinction between a tree that sheds its leaves in the autumn and regains them in the spring, and another that retains its leaves all the year round. The internal system of such trees is manifestly widely different. Yet we have an example of a tree, the plane-tree, occasionally varying by becoming evergreen. One such in the island of Crete was famous

in Pliny's days and for long afterwards; then it disappeared; **within the last few years** it has reappeared, fresh shoots out of an old trunk cut down (which does not seem when standing to have been evergreen), again showing the old characteristic. This variety, we are told, could not be propagated elsewhere, the seedlings withering everywhere but in their native spot, no doubt from lack of some peculiarity in the soil or situation. We can, however, readily believe that, had the appropriate soil and situation been plentiful, this variety might have turned out its progenitor, have become confirmed in its difference, and been ranked as a remarkably distinct species.

But further, variation extends also to instincts and habits of life; acquired instincts are hereditary quite as much as natural ones. The case of the *pointer* is an excellent instance, the instinct of pointing being one known sometimes to occur as a variation, and being also one certainly transmissible by inheritance. It is highly probable that it was indeed originally nothing but an individual variation, become now by selection and inheritance the permanent characteristic of a race. Variations of habits in domestic animals, such as different degrees of docility, preference for particular kinds of food, fondness for various pursuits, &c., are too familiar to every one to need especially insisting on. Nor is there any doubt that such variations are to some extent hereditary. To take but a single instance, this time from creatures in a state of nature:—the dread of man, undoubtedly hereditary in many wild species, is shown, by the experience obtained in newly-peopled islands, to be an acquired, not an original instinct.

In the same way as instincts and habits may thus be occasioned by change of circumstances, as well as by natural variability, so may differences in structure and development be brought about by altered conditions of life. Every one knows how largely use and disuse tend to modify powers; few, however, probably realize the extent to which this involves also modification of structure. The superior use of the right hand and arm in man renders it the strongest and most adaptive; it also lengthens and enlarges the bones composing them. Continual practice in running will conduce to greater fleetness, which again depends in part upon the relative size of certain bones. Not only are persons born with short sight and long sight, but these can also be acquired by use. The sailor, habitually accustomed to descry distant objects, lengthens his sight, can see farther than others. The student, used to poring over his book, shortens his sight, can see nearer than others, but at the same time loses his power of seeing far off. Now what does this involve? There is in the eye a wonderful power

of altering the focus of the lenses so as to suit divers distances. This power is, however, not only naturally variable, but so little constant as to be considerably altered by use in a particular direction, even through a comparatively short space of time. It can be increased in either direction; but this increase brings with it a corresponding limitation in the other. Similar alterations in structure by use in respect to the ear might easily be instanced. So, again, there is no doubt that the size and proportional development of the stomach and other internal organs are directly influenced by the nature of the food habitually eaten; the size of the lungs by the temperature of the air breathed, and the occupation of their possessor.

These are the kind of elements with which natural selection has to deal: differences in structure, physiological character, instinct, and habit; differences, some of them directly occasioned by the conditions of life, the use and disuse of particular organs, some of them by causes existent before birth, and of which we know nothing; differences all certainly transmissible by inheritance. To appreciate the wide extent of the ground covered by such known individual variations would require a detailed survey of facts infinitely fuller than the bare outline here afforded, which, as already remarked, merely professes to give illustrations of the different *kinds* of variations observable. We may, however, even from this meagre view, unhesitatingly conclude thus much:—that there is no *class* of specific differences which facts do not fully warrant us in regarding as possibly caused by inherited variations. The *amount* of such differences will come under consideration further on.

But now, in the next place, of the power and extent of natural selection as a process for preserving and confirming such variations. Here we need carefully to bear in mind the exceedingly complex relations in which all living beings stand. There are first their relations to inanimate nature, to soil, climate, and situation. Then there are their direct relations to one another, the presence of one being necessary to the well-being or existence of another, or acting as a check upon its development; so that the increase or decrease of one will entail at once the increase or decrease of the other. Then, thirdly, there are their indirect relations, caused by that competition of races and individuals before dwelt upon; those which do not directly affect each other's well-being, yet struggling together for existence, by reason of a greater number of germs being constantly produced than can possibly attain to maturity. These various relations affect species in every part of their

being, their structure, their physiology, and their habits; affect them also at different periods of their life in a different way, and in regard to different elements in their constitution. Every species being thus subject throughout its whole life to an immense variety of stringent tests by which its relative predominance is determined, the result of this must plainly be, on the average, the maintaining each species at its highest pitch of perfection in respect to the particular conditions to which it is exposed. With this, however, will always be also a distinct tendency to preserve variations, even of the slightest kind. In any given area the largest number of individuals, whether of plants or animals, will be found capable of co-existing, when the differences between them are at their greatest. It is well known, for instance, that a heavier crop of hay is obtained from a field sown with mixed seed than from one sown with only a single kind, simply because in the former instance more individual plants are capable of growing together than in the latter. The tendency of the struggle for existence being, of course, to preserve in every case the largest number of individuals possible, there will thus be an intrinsic advantage in every variation, apart from any positive bearing it may have on the well-being of the species. The severity of the struggle with individuals of the same species will at all events be diminished, and so a greater chance of preservation be afforded. Thus, even supposing no change to take place in the conditions of life, it is quite credible that natural selection should so seize hold of and confirm even indifferent variations as to make them permanent. How much more if they are of a kind directly profitable.

But the conditions of life do not remain unchanged. The development of a new variety or species in the manner just noticed, the diminution or extinction of another by deterioration (for species certainly vary in both directions), the immigration or chance introduction of some foreigner previously unknown there, would at once alter the relations of each species to the other, and so affect the kind of test by which their predominance was determined. Irregularity in the seasons might give especial advantage to some individuals and races, especial disadvantages to others, and thus tend to extinguish certain variations and preserve others, besides leading to internal alterations of relation. Changes in physical geography brought about by geological forces would be still more potent, as producing differences in the conditions of life more permanent and extensive. A greater or less elevation, an altered flow of rivers, a different course of ocean currents, the connection or disconnection of land with land,—all would

bring with them most important changes in the conditions of life, and so a new set of tests by which natural selection should work, leading to the extinction of existing races and the development out of the surviving ones of new. Which changes, moreover, as Captain Maury especially has well shown, would by no means be confined to the particular places where the geological forces were actually at work, but, from the extremely complex relations in which all parts of Nature stand to one another, would extend their influence more or less over the whole earth.

Taking all these circumstances into account, then, the conclusion seems plain, that there are in the diverse and exceedingly intricate conditions of life to which all species are exposed, in different places and at different times, amply sufficient points of contact between natural selection and inheritable variation to account for variations of every kind being taken hold of and preserved in such a way as eventually to lead to their appearance as specific differences. This being so, the inference would at once arise (bearing in mind the former conclusion as to the kinds of variation actually observable), that the Darwinian hypothesis of the origin of species is capable of accounting for every kind of specific difference known to exist; that is, that it is *adequate*.

Before, however, fully endorsing this conclusion, it will be necessary to consider that further point alluded to above, the *amount* of the differences. Now, taking the hypothesis as it stands, that all past and existing species of living beings inhabiting the earth have sprung from at most some eight or ten original forms, the amount of difference does seem overwhelmingly enormous. To suppose, for example, that all vertebrate animals, or all exogenous plants, have descended from the same progenitor, is an immense exertion for the imagination. But how does Mr. Darwin's hypothesis suppose this transformation and development to have taken place? By single strides? No; but by an exceedingly long series of exceedingly small steps. A traveller standing at the foot of Mont Blanc, viewing through his telescope another who had reached the top, and then scanning the marvellous obstacles of mountain peaks, precipices, and glaciers that lay between, might be disposed to say that it was impossible for any one to climb from where he stood to that lofty summit. To the imagination, merely taking into account the enormous height, the apparently insuperable hindrances, it might well seem so; and yet step by step, through long and often circuitous paths, round obstacles, if not over them, it could be done. Just so with Darwinism. Not by a sudden transition from class to

class or order to order, but by the change of species into varieties, varieties into species, each transition involving no greater alteration than is known in such cases often actually to take place, the transformation of the one into the many *could* have taken place. Granted, then, that there was time enough for such slow development to have produced the effects we see; granted that the conditions of life have varied in different places and times to a sufficient extent to cause natural selection to have been carried on in exceedingly diverse directions; and there is nothing in the *amount* of the differences, as distinguished from their kind, which presents any valid obstacle to the adequacy of Darwinism. That the conditions of life have thus been perpetually varying, the testimony of geology assures us in the plainest terms. That the time has been enormously long, is, according to most geologists, equally certain; while those who dispute the assertion do so, not by producing positive evidence that it was actually short, but by rebutting their opponents' arguments, by showing merely that it *need* not have been long. Still, therefore, even if the position of these be admitted as well established, it remains an open question whether, after all, the time *may* not have been amply long enough for all to have occurred which Mr. Darwin's hypothesis requires.

One further remark only is necessary before leaving this part of the subject. It is by no means to be imagined that every difference now distinguishing species from species was seized hold of by that natural selection which led to their separation. The principle of correlation of growth, on which the whole science of comparative anatomy and palæontology depends, tells us that a difference in any one member involves also differences in other and related members, so that from a tooth only the whole structure of an animal may be inferred. The particular point of variation on which natural selection seized, might thus be but a single element in the total of differences that ultimately characterized the species, the remainder being the result of correlation. This should ever be borne in mind when inquiring into the possible way in which particular characteristics could have been exposed to the influence of natural selection. They may never have been exposed to it at all, but be the correlated results of other and far less apparent differences, which were so exposed.*

3. We pass, then, thirdly, to the *consistency* of the hypo-

* From here to the end of the paper was delivered extempore, being written out afterwards. No attempt has been made to preserve the original phraseology in thus reproducing it; the matter and arrangement have, however, been strictly adhered to.

thesis. Are all the phenomena observable in Nature, which should be if Darwinism be true? The extent and pretension of the hypothesis expose it to the severest possible tests in this respect. If all living beings have indeed descended from a very few original progenitors, then there must flow from this certain well-marked characteristic in respect (1) to their present nature and relations, (2) to their distribution in space, and (3) to their distribution in time. Under these three heads, then, I propose to consider the most important of the tests of consistency to which Darwinism is fairly subject, confining myself as before to a general outline, without entering fully into details.

First, then, of the present characteristics of living beings, their nature and relations to one another. The principle of Darwinism being the development of a vast number of forms by gradual divergence from a single original, it follows at once that if the hypothesis be true, all such forms should be capable of being arranged in groups of continually increasing diversity, retaining, however, even to the last, some characteristics in common. Not only should we expect to find in every species resemblances to the first progenitor of the whole, but also resemblances to the various intermediate members of the series, these resemblances increasing as the line approached its termination; the whole sum of characteristics possessed by each species being the result of successive additions upon those common characteristics belonging to the whole class. And precisely so we find it. The whole natural system of classification is based upon this principle of group within group; first species, then genera, then sub-orders, orders, sub-classes, classes; the amount in common constantly diminishing as we ascend, yet something, and often a good deal, in common being found even to the last. This latter is an important point. Look for example at two of the great classes of plants, exogens and endogens;—what a group of characteristics does each present. Here are the exogens, growing by the formation of additional layers outside the old, possessed of two cotyledons in their seed, having leaves with reticulated veins, and flowers with the parts most commonly in multiples of four or five. Here are the endogens, growing by addition to the inside, possessed of but one cotyledon, having leaves with parallel veins, and flowers with the parts in multiples of three. And these characteristics are common to all the myriads of orders, genera, and species which each class contains. All exactly as was to be expected if the Darwinian hypothesis were true.

Then, further, it is to be noted that the distinction between

these successive groups is purely arbitrary. The division of classes into orders, and these into sub-orders and genera, is highly convenient, and as already noticed, on a general view, not without strong warrant of facts in its favour; but in its precise limitations it is arbitrary. Naturalists are perpetually divided not only as to which are species and which varieties, but as to where genera begin and end, how far orders and sub-orders are to be distinguished, and especially under what head particular species or genera are to be ranked. The constantly increasing divergence that appears as we ascend the scale almost necessitates such intermediate groups being introduced, and yet the gradations are in many cases so fine, the connecting links so numerous, as to render it a difficult if not a hopeless task to define and arrange these groups in a perfectly natural manner. Again, precisely what might have been expected if all these successive groups were the irregularly divergent but yet related descendants of a single progenitor.

Once more, it is to be noted that the differences which distinguish these various grades of groups from one another vary exceedingly as to the organs and characters which they concern. Now it is the most important which are found to differ, now the least; nor does this variation accord in any way with the importance of the classificatory distinction. Thus we have some orders of plants (as *Cruciferae*) where the number and position of the stamens, the arrangement of the petals, &c., are alike throughout; the generic and specific characters being obtained for the most part from organs of less importance. And again, we have other orders (as *Connaraceae*), where the most radical characters are found to vary between genus and genus; or in some cases even between species and species. Had the contrary been the case, and the most fundamental organs afforded the characteristics of the larger groups, the less fundamental those of the subordinate ones, and so on in regular gradation,—had this been so, the arrangement and relations of living beings would have presented a symmetry and manifest method strongly suggestive of especial design and arbitrary plan. The opposite to this, however,—irregularity, ununiformity, apparent lawlessness,—was naturally to be expected, if all these groups were really the diversified offspring of a common parent, since such diversification would be certain to proceed irregularly in different directions. And exactly thus we find it.

We come now to another test. If the Darwinian hypothesis be true, then not only have large groups of species descended from single progenitors, but the mode of descent has been by

an enormous number of intermediate forms. Are such intermediate forms to be found? Here we must, in the first place, inquire how far, supposing the hypothesis true, it were to be expected that they should be found. The mode of production alleged is a seizing hold by natural selection of profitable variations in individuals tending to the preservation of such to the exclusion of others. The same power that determines the greater predominance of the variant determines also the less predominance of the non-variant; so that if the variation be important, its preservation and confirmation carries with it, of necessity, the ultimate extinction both of the original, and also of the successive steps by which the full extent of variation was attained. It is thus a necessary consequence of Darwinism that at no one time should a large number of intermediate forms be found co-existing. Only in the case of indifferent variations not much affected by natural selection, or of other variations in particular stages of their progress, was it to be expected that such forms would be found. Their presence would be the exception, their absence the rule. And just so is it found to be in fact. Here and there are cases (*e.g.* the brambles) where intermediate varieties are so numerous and so finely transitional as to make it almost impossible to determine which are species and which not. In the majority of cases there is no such difficulty, but the specific differences are clearly marked. Again, precisely what the Darwinian hypothesis would have led us to expect.

Yet another test. If all existing species are the descendants of other and different species, it is natural to expect to find in them various marks of this descent over and above those common characteristics of classes, orders, and genera before alluded to; these marks varying in character according to the remoteness of the ancestor whom they concern. Thus it is well known that in artificial breeds there is an occasional tendency to revert to the peculiarities of the original stock, and this especially when several distinct breeds are intercrossed, and the variations of each thus neutralized by intermixture. The instance of the pigeons given by Mr. Darwin* will occur to every one who has read his book. The like reversion might naturally, then, be expected to take place among species in nature. And the facts accumulated by Mr. Darwin touching the occasional appearance of stripes and bars on various species of the horse genus, and especially on hybrids between any two of them,† show unmistakeably that the same kind of phenomena does, in fact, occur here also.

* Pp. 26-7.

† Pp. 191-5.

Reversion is of course only to be expected where the character reverted to belongs to a comparatively recent ancestor. Another mark of descent, reaching further back, is the presence of organs in a disused or rudimentary condition which formerly were of importance. When any organ becomes, from changes in the conditions of life, unused, the most probable result would be that it should gradually become less and less perfectly developed; at the same time it is quite conceivable that it should be retained for some time fully developed, though no longer of use. Both cases are found in nature, the latter occasionally, as in the geese with webbed feet who never go into the water, and the woodpecker who never climbs a tree; the former frequently, as in the rudimentary teeth of whales, the rudimentary tail in tailless animals, the rudimentary wings of the apteryx or ostrich, the rudimentary stamens in female flowers, &c. Both manifestly present great difficulties on the ordinary theory of special creations, but fit in naturally with the Darwinian hypothesis of irregularly diverging common descent.

Then to go a step farther back yet. Not only have we disused and rudimentary organs, but also organs differing enormously in development and use, yet radically identical, or even capable of transformation into one another. Thus the wing of a bird, the arm of a man, the paw of a lion, the flipper of a seal, are all strictly homologous structures, made up of similarly related and connected bones, though externally so exceedingly different. Thus, again, in plants the different parts of the flower are seen occasionally to turn into mere leaves, showing the morphological identity of these so diverse organisms; while in some cases, as the white water-lily, the transition from sepals to petals, and from petals to stamens, may be seen in all its fine gradations even in a single flower. All this is of course just what was to have been expected, if the Darwinian hypothesis of the common origin of species having homologous structures, and the enormous capability of variation possessed by every part, be accepted as true. On any other theory such phenomena are simply curious but inexplicable facts.

Lastly, as the deepest-seated and farthest-reaching of all these marks of descent, we have the phenomena of embryology. It was to be expected that if whole groups of living beings have really descended from a common progenitor by subsequent variation, the differences thus resulting should be developed in each individual somewhat later in time than those fundamental characters which all inherit in common; in other words, that in the first stages of growth there should

be more resemblance between such related species than subsequently. The investigations into the gradual growth of embryos before birth show us that facts fully answer to this expectation. The differences between the members of the same class are slowly built up by the diverse development of forms at first utterly undistinguishable; and the more nearly allied the members are, the later do the differences between them appear.

The remaining test belonging to the head of present characteristics is one of an entirely different kind, which affords a natural transition to the next division concerning distribution in space. If the effect of natural selection upon species exposed to it be to preserve and perpetuate their most improved forms, it follows at once that in those places where natural selection is carried on most vigorously, there should the species be most improved. The severity of the selection depends mainly upon the amount of competition to which each living being is exposed; clearly, then, in wide-spread areas, where there are a large number both of races and individuals struggling together, it was to be expected that both improvement and extinction should go on most rapidly; in confined and isolated areas, where the races and individuals are fewer, it was to be expected that both these processes would go on much more slowly. And precisely so we find it. Isolated localities—as islands, fresh-water lakes, caves, &c.—are ever found to present the greatest number of peculiar forms, often so resembling bygone types as to receive the name of “living fossils.” While, if the comparative improved condition of the species generally be inquired after, it needs but to put the flora and fauna of an isolated and extended area into actual competition, the result speaks for itself. The species from the latter, if introduced into the former, speedily supplant and extinguish the greater part of them, while those from the former are altogether unable to retaliate if transferred to the latter.

We come now to the second division of tests of consistency, those, namely, which concern *distribution in space*,—tests perhaps the severest of any to which the hypothesis is subject. Darwinism supposes that every species of a genus has descended from an original single species; that every such representative species in each order has descended in like manner from one original, and so on. But these species and genera are scattered in all directions over the face of the globe. It is incumbent on the upholders of Darwinism to show, then, (1) how the original representative species could have become so distributed as that their varied descendants should appear in the places they now do; and (2) that the systematic

affinities of the flora and fauna of different places accord with the mode of origination thus assumed.

First, then of the means of dispersal. To enter into this at all at length would require the whole evening; it must suffice, therefore, to allude to a couple of instances of a very different but equally important character, by way of illustration. The close affinity between the Arctic flora on high mountains in all parts of the world, however remote, appears a case of peculiar difficulty. How can the supposed common progenitors of these nearly allied or even identical species, so different from those existing in the adjoining temperate or tropical countries, have become distributed into their several places? The answer is found in the prevalence at a comparatively recent period of great cold over large portions of the earth's surface, accompanied with glaciers and other Arctic phenomena. Such increased cold would naturally drive the Arctic flora of the north pole southwards in all directions over districts now utterly uncongenial to it. On the diminution of the cold, this flora would plainly retire not only northwards, but also up the mountains in all parts, the congenial portions of which, now so completely isolated, would thus be clothed everywhere with species drawn from a common source, exactly as we should surmise to have been the case from their intimate systematic relations. This instance is one where great apparent difficulty is turned into confirmation. The second is one which on the face of it remarkably confirms the hypothesis of common descent. Oceanic islands, if not peopled by special creation, can only conceivably have been peopled by birds, insects, seeds, &c., having been either blown or washed thither. Only some species, plainly, could thus be conveyed—*e.g.* of land mammals, only those which could fly, namely *bats*. It is a remarkable fact that the only mammals that are found on such islands (*i.e.* those very far removed from the mainland) are precisely bats, just as this theory of distribution would require. But further, these bats are in many cases of peculiar species, found nowhere but in their several islands, exactly as might have been expected if they were the descendants of isolated individuals long ago blown thither. That they should be thus peculiar, and the only mammals found there, though others are fully as capable of living there, are facts alike inexplicable on the theory of special creations.

But, secondly, of the relation between geographical connection and the affinities of flora and fauna. This appears in many ways. Thus the species existing in different islands of a group, though often very distinct, are always more nearly related to one another than to those on the mainland. The flora and

fauna of islands resemble most closely those of the nearest continent, with a few exceptions, where ocean currents or prevalent winds afford especial means of transit from other sources. The flora and fauna of whole continents, as America, present similar internal affinities, though spread over areas most diverse in situation and climate. The same truth appears also in the marvellous effect of great natural boundaries, as impassable isthmuses, however narrow, and deep sea-beds, in regulating the affinities of marine fauna. Lastly, the same is seen also when we turn to the records of geology, the fossil remains of the old and new worlds for instance, presenting similar, though somewhat less marked, differences with those observable in the living species. It is, not, of course, pretended that these facts afford any direct argument in favour of Darwinism; they are merely so many tests which it must answer satisfactorily in order to be established as consistent. The fact that it does answer them is of value as an argument only by reason of the number and severity of tests, it being improbable that an untrue hypothesis should not somewhere be caught tripping.

The last item considered—the analogy between the fossil remains and existing species of the same areas—leads naturally to the last division of these tests of consistency, those, namely, which concern distribution in time. Unwarrantable as it was shown to be to expect a large number of intermediate forms to be found co-existing at any one time, it is clear that if Darwinism be true, such intermediate forms in innumerable hosts must have existed, now here and now there, in days gone by. Surely, then, we ought to find the proof that they did so exist in the remains preserved to us in the rocks. Now that these remains prove that, for the most part, different species formerly lived upon the earth from those now inhabiting it, and that this difference steadily increases as the strata examined are more and more remote,—this geology proves incontestably. Still we have no such enormous number of strictly intermediate forms as might, *à priori*, have been expected. How is this? In the first place, it may be asked, how far is it really reasonable to expect that such intermediate forms should be preserved? Geological formations are undoubtedly going on at the present time; changes in species, at all events in domestic breeds, are also going on; how far, then, would these changes be perceptible in the formations? But rarely, and as it were by chance, do any remains of these animals or plants become entombed at all. Now and then a skeleton or some stray bones may be carried away by a river, or become embedded in sand or mud, not however without enormous risks of total disintegration; now and then a fallen

tree may sink into a muddy lake or bog and be preserved, if speedily enough excluded from the air; but how utterly inadequate would such occasional entombments be to afford an index of the whole existing fauna and flora,—how absurdly insufficient for pronouncing as to whether any changes in these or other species had been going on during the time of such formations. We may safely assert that the geological formations now being produced could only most exceptionably give any indication of the truth of Mr. Darwin's hypothesis, supposing that hypothesis to be true. On what principle and by what right, then, are we entitled to expect that past formations shall do so? and why should we regard their not doing so as an objection to the truth of the hypothesis? We know, again, that at the present time geological formations are purely local, and probably temporary, so that only a few parts of the whole earth's surface would have the remotest chance of having their inhabitants preserved. Is it not probable that the formations we now have in the rocks were equally local and equally temporary? Once more, we know that notable changes in the flora and fauna of places are often produced by the immigration of species from elsewhere, who supplant and extinguish the old ones. Is it fair, then, to ascribe similar sudden changes in the fossil remains of successive layers of deposits invariably to new creations? In a word, taking known facts touching present geologic changes as our guide, not one of the fancied objections to Darwinism drawn from the geologic records of the past can be allowed the slightest weight. It is most unreasonable to expect that there should be preserved in the rocks the innumerable intermediate forms which the Darwinian hypothesis requires, because of the extremely small proportion of formerly existing living beings possibly entombed there, and the probably local and temporary character of the deposits; while the difficulty which the sudden appearance of new species and groups of species is thought to present, falls to the ground at once when the known results of immigration are further borne in mind. And here we may fairly turn the question the other way, and ask what geological evidence would satisfy an anti-Darwinian? Suppose a series of intermediate varieties were shown linking together two successive species, what would he say to it? Why, that they were not distinct species at all, but merely varieties; or if the grades were a little less fine, that every one of the intermediate forms was itself a specially created and immutable species. The foregone conclusion would colour everything.

But, secondly, what evidence bearing upon the subject does geology really afford? It shows us an immense number of

additional species, all referable to the same great classes as those now existing, and mostly referable to the same orders also. It shows us that these species were most like in those periods of the earth's past existence nearest to each other in point of time, most unlike in those most remote. It gives us the clearest proof of gradual alteration in the predominant species from period to period, or even within the same period, each strata and each layer of strata being on the whole intermediate in character between those immediately above and below. It gives us especially a number of most valuable additional links in the chain of being, which tend to bring genus and genus, order and order, class and class, ever nearer and nearer to each other. In a word, its whole evidence is, considering its imperfect character, precisely what the Darwinian hypothesis would have led us to expect.

Thus on every hand, and in every possible way, the consistency of the theory is tried, and still it stands the test. In many respects, no doubt, the evidence at our disposal is insufficient to warrant definite conclusions; in others the consistency is rather hypothetically possible than demonstrably certain; but in no respect does there seem the slightest reason to pronounce it certainly inconsistent.

4. It remains only now to apply the last inquiry concerning the hypothesis;—is it *harmonious*? It is of course conceivable for an hypothesis to be both possible, adequate, and sufficient, so far as our evidence goes, and yet not be true. It is asked, then, is there any ground of analogy to render it probable that Darwinism, if it eventually answers these three main requirements, is the true explanation of the phenomena in question? In other words, is the method in which it asserts species to have originated one which there is reason to regard as in accordance with the ordinary and known workings of God? Here, then, we come to the Theology of Darwinism. Its relations to Scripture I purposely pass by, for I do not believe that Scripture was ever meant to teach us science, and hence that the less they are brought into comparison, the better for each. But as regards its Theology, I make two remarks. In the first place, it assumes no cause, force, or influence other than those known to be at work at the present day. By growth and reproduction, all living beings now propagate themselves, by inheritance they communicate their characteristics to their descendants, by natural selection the predominance of race and individual are determined; by these, co-working with variation, *some* changes at all events, be they few or many, be they great or small, are unquestionably produced. All that Darwinism requires of us is to be-

lieve that thus it has always been from the time when God first created living beings on the earth, and that to these causes are to be ascribed all the changes in such beings subsequently introduced. Looking at it in this light, I confess myself utterly at a loss to understand how any objection can possibly be taken to Darwinism theologically. We believe that all living things we now see about us were made by God, by means and under the influence of these causes involved in Darwinism; we feel no difficulty in so believing; why, then, should we feel difficulty in so believing as to all living things in the past? Nay, the analogy goes further yet. For if it be once established that the causes involved in Darwinism are adequate to produce the effects ascribed to them, then, being causes undoubtedly employed by God as instruments at the present time, there is at once the strongest possible presumption that they were the causes employed by Him in time past.

The second ground of analogy to which I would refer concerns the corresponding alterations in inanimate nature. The soil, the climate, the relations of sea and land, have differed as widely in bygone times from what they now are, as the species conditioned by them; they have changed, often contemporaneously as these have changed. In what manner do we conceive that these changes were brought about?—by miracles? No; but by the working of the same laws and forces as are at work at the present day. Darwinism, then, simply asks us to regard God's method of effecting changes in living beings as the same with His method of effecting changes in inanimate nature,—transition, extinction, development,—not fresh creation.

In conclusion, I would say that, as every one acquainted with Mr. Darwin's book will have seen long ago, the present paper makes no claim to originality. It is simply an attempt to exhibit in a concise form the logical value of the most important arguments adduced by Mr. Darwin, and the inference to be deduced from them. What that inference is, cannot, I think, be mistaken by any one who has followed the line of reasoning pursued. It is that Darwinism, though very far from being established as a true hypothesis, owing to lack of evidence in many important particulars, is yet supported by so strong an array of testimony of all kinds as to be certainly *credible*, and so a good working hypothesis for investigators to keep in mind. Mr. Darwin's own book is professedly but a meagre abstract of the evidence on behalf of the hypothesis he has in store. The full statement has long been promised, and, in respect to one important part of the subject, is announced as now "preparing for publication." It were

rashness in the extreme to jump to any definite conclusion until this fuller statement has been seen and weighed. And even then much further investigation into facts will probably be needed before a final decision can be made. Meanwhile, I submit that Darwinism is certainly to be maintained as *credible*.

THE CHAIRMAN.—I think you will all agree with me in passing a vote of thanks to Mr. Warington for the very admirable and distinct manner in which he has stated the arguments of Darwin. I think, whether we agree or disagree with Mr. Warington, we must be very much indebted to him for the lucid manner in which he has done this ; and I will go so far as to say that I think he has done more justice to Darwinism than the book of Darwin himself. We have thought it expedient in a matter of this kind, particularly as the paper is not quite finished, and was not laid before the Council before it was read, that all discussion upon the subject should be deferred till another meeting. I may say that the paper is worthy of fair discussion, and I do not think it would be fair to discuss it without full preparation. Mr. Warington has stated the thing so clearly and systematically that as an anti-Darwinian I am much obliged to him ; for it has only proved to me, if I may venture to express my humble opinion, that Darwinism is not a bit more credible than I thought it was before. But that is a matter on which persons have a right to form their own opinion ; and Mr. Warington has put the matter in such a plain, logical, and dispassionate manner, as fairly to open up the question for future discussion, and in doing so I think he has done good service to the Victoria Institute.

MR. REDDIE.—I beg leave to announce that I have in my possession the first part of Mr. Warington's paper, which has been already written out, and that it will be in the printer's hands to-morrow morning. I may also venture to say—since Mr. Warington has promised me the remainder of the paper in a day or two—that copies of the whole paper will be in print and ready for distribution, to members who may wish to join in the discussion, by Saturday morning next. Sir, I cannot sit down without expressing how cordially I concur in your commendation of the clearness of Mr. Warington's paper, and in the vote of thanks to him for it. I especially wish to say this, because, as an anti-Darwinian, I must add, that I have not been in the least convinced by anything that Mr. Warington has advanced. On the contrary, after hearing his arguments, I feel if possible only the more persuaded that the theory of Mr. Darwin is *inharmonious, inadequate, inconsistent, and utterly incredible*. (Hear, hear.)

The Meeting was then adjourned.

ORDINARY MEETING, MARCH 18, 1867.

THE REV. WALTER MITCHELL, VICE-PRESIDENT, IN THE CHAIR.

The minutes of the previous Meeting having been read and confirmed, it was announced that G. T. Miller, Esq., 59, Portland Place, had been elected a Member of the Institute.

The discussion upon Mr. Warington's Paper, read at the last Meeting, "On the Credibility of Darwinism," was resumed by Mr. Reddie; who read the following Paper in reply to that of Mr. Warington:—

ON THE CREDIBILITY OF DARWINISM. (In reply to Mr. Warington's Paper, read March 4th, 1867.) By JAMES REDDIE, Esq., Hon. Sec. Vict. Inst.

ON the present occasion, Sir, I could have wished that Mr. Warington and myself had changed places. I almost wish, I mean, that I could have written and read his paper, that I might have had the satisfaction of hearing how he would have criticised it. He will not, I hope, misunderstand the double compliment I mean most sincerely to pay him, in saying this now. Could I have undertaken to write in defence of Darwinism, I would have wished to write as plainly as Mr. Warington has done. And if I wished, on the other hand, to pull all the arguments he has advanced to pieces, I should like nothing better than to let loose *his* critical faculty upon the paper it is now our duty to discuss. I think, Sir, it is a happy circumstance that in this Society such an impartial and temperate paper should have been read upon such a subject; and I most sincerely trust that the tone of the discussion throughout will be that observed by Mr. Warington, whether we agree or disagree with the views he has advanced. I have thought it right to make these preliminary remarks, all the more because I so thoroughly *disagree* with Mr. Warington

from first to last, and am now about to move, as it were (as they say "in another place"), a direct negative to all the principles, assumptions and arguments throughout his paper. I must, however, reverse his way of putting the subject before you. I think Darwinism incredible, not because I can first prove it to be impossible, but because I hope to show that it is inharmonious, inconsistent and inadequate; and that it is therefore, if not "impossible," yet utterly improbable, and that it ought to be at once rejected as an *irrational* hypothesis, and altogether *incredible*. You will observe that I disclaim being able logically to prove that Darwinism is "impossible," while Mr. Warington has boldly claimed to have proved it to be possible. Well, Sir, in my opinion he has gone quite beyond the range of *à priori* possibility in the case, in even attempting to do what he thus has claimed to have done. I can perfectly understand his *believing* the theory to be possible as he has put it before us. Darwinism *plus* Deity must, no doubt, be possible as a mere conception of the mind,—*i. e.*, if we assume that God has chosen so to work; but Darwinism, pure and simple, as the French say, is a very different matter. Nor must Mr. Warington object to my drawing this distinction. I assure him I intend to steer clear of all *odium theologicum*—as I trust others will of all *odium scientificum*—in discussing this vital question; but at the same time I have no intention of avoiding—and I am sure it will not be expected that I should avoid—speaking perfectly freely on the subject, and bringing out the logical issues to which the hypothesis leads, not only in my opinion as its opponent, but in that of some of its own most zealous advocates. At the same time I beg to say that I shall touch very lightly upon that most important issue, and as far as possible (in order to do mere justice to the argument) I shall limit myself to the issues raised by Mr. Warington himself. I shall do so, if for no other reason, because, from past experience in discussing Darwinism with others, I know how skittish Darwinians can be; and I wish to impress it upon the members of the Institute that they must not conclude, even if we refute Mr. Warington, that it will be admitted we have refuted Darwinism, but only *his* way of supporting it. Even Mr. Warington himself frankly tells us in the concluding sentences of his able paper, that "Mr. Darwin's own book is professedly but a meagre abstract of the evidence on behalf of the hypothesis he has in store. The full statement has long been promised, and, in respect to one important part of the subject, is announced as now 'preparing for publication.' It were rashness in the extreme to jump to any definite conclusion until this

fuller statement has been seen and weighed. And even then much further investigation into facts will probably be needed before a final decision can be made." "Meanwhile," Mr. Warrington—with, in my opinion, the extreme rashness he has thus very sensibly deprecated—does "jump to a conclusion," in the absence of the coming evidence, and "submits that Darwinism is *certainly* to be maintained as *credible*."

I have said that I shall reverse the order, as well as endeavour to negative the conclusions, of Mr. Warrington's several propositions. But in the first place I must touch upon his preliminary matter—his principles of philosophizing and the analogy he adduces—before entering upon the more immediate question he has brought before us. Well, Sir, here again, I am unfortunately at issue with Mr. Warrington in some important respects. He appears to me to have quite thrown over the very principles of inductive science in his opening sentences. He is positively in love with hypothesis, theorizing and speculation. We need not, therefore, be surprised that "to love and be wise" has been beyond his power. He concludes that mainly, if not exclusively, "it is through hypothesis that truth is ultimately attained;" and not only so, but throwing Bacon's cautious and philosophic wisdom to the winds, he actually believes that we positively cannot collect together and store up a knowledge of the facts of nature, without first of all determining "*what facts* especially need to be accumulated and sought after." This mode of collecting facts which have been sought after in order to meet the needs of a foregone conclusion, must remind us of the temple, alluded to by Bacon, in which were to be found the votive tablets of those who had escaped the peril of shipwreck, and which were appealed to as proving the power of the gods to which they had been offered, but where the portraits of those who had perished, after making the very same vows, were altogether absent. (*Nov. Org.*, i. 46.) We have had some experience, too, since Bacon's day, of the effect of this method of seeking for and tabulating facts to suit some favourite hypothesis. And I have sufficiently expressed my opinion of the vicious nature of this unphilosophical mode of "going on for years collecting and arranging in the mind all newly-discovered facts, with sole reference, for instance, to the nebular hypothesis," only recently given up.* But still I agree with Mr. Warrington to this extent, that men are prone to theorize and speculate, though in my opinion they often do so in

* *Scientia Scientiar.*; *Journ. of Trans. of Vict. Inst.*, vol. i. p. 21.

detriment to the true advancement of real science and in spite of all Bacon's principles and warnings. And that being the case, I quite accept as a necessity that we must look these theoretical speculations in the face, and, if we can, refute them.

I now come to the consideration of the analogy of the theory of universal gravitation, adduced by Mr. Warington as an example for our guidance in testing Darwinism, as he evidently intends it should be tested, by what he considers the most rigid of scientific tests. In my opinion, this analogy has been most happily chosen. Chosen happily by Mr. Warington, because the choice proves how thoroughly he means to test the theory the credibility of which he pleads for. Chosen happily, also, Sir, because you preside over our deliberations, who are most competent to estimate both the abstract and the relative merits of the proofs relied upon for the establishment of the two theories thus placed in comparison. And happily chosen, I beg leave to add besides, on my own account, because of the way in which my name has recently been publicly mixed up with the Newtonian hypothesis in connection with this society. I allude to an article especially in the *Saturday Review* of 12th January last, and I am glad of the opportunity now given me to show to our members that I have some reason on my side. The theory of universal gravitation being a subject to which, like yourself, I have given considerable attention, (though we have viewed it from different stand-points—I as a sceptic, and you as a believer, —and at present, perhaps, we have therefore naturally arrived at different results,) I am able to say that the analogy sought to be established by Mr. Warington is probably much more applicable than he imagined to the theory of Mr. Darwin. Only in the first test does the analogy entirely break down. We can prove or disprove, by absolute mathematical demonstration, the possibility of universal gravitation. But, as I have already said, this we certainly cannot do with respect to Darwinism. But as regards the other three tests—adequacy, consistency, harmoniousness—the analogy “runs on all fours.” When once we get over the question of “possibility,” these tests can be applied equally to both the hypotheses. Before, however, I proceed to examine how these tests have been or may be applied to Darwinism, there is a prior part of the analogy to be glanced at. We must not forget, then, that the present distinguished naturalist, Mr. Charles Darwin, is not the first propounder of what we now call “Darwinism.” I am not even quite sure that the theory of “natural selection,”—as explanatory of the resultant hypothesis of developmental transmutation of species,—can fairly be attributed to him as its sole

author, except as regards this new name, he has no doubt furnished the theory. But, at any rate, his grandfather, Dr. Darwin, preceded him; as did also Lamarck and Monboddo, to mention no other more ancient but less-known names, who have held the same views as regards all essential results, though they failed to give precisely his explanations of how the results were brought about. In the notorious anonymous volume, *The Vestiges of Creation*, we had essential Darwinism put forward most confidently, without Mr. Darwin's carefully selected and ingeniously varied and modified explanations; which have since been developed, in support, however, we must always remember, of conclusions arrived at previously. But Dr. Louis Büchner, in his *Kraft und Stoff*, distinctly claims to have put forward views identical with those of Mr. Darwin seven years before *The Origin of Species by Natural Selection* was published, though he recognizes the value of the "most convincing proofs" which he says Mr. Darwin has furnished in support of those views. (*Force and Matter*, p. 91, note.) Well, we have a very close analogy to this in the history of universal gravitation. On a recent occasion, when Dr. Gladstone read a paper here, I pointed out, by citations from the *Philosophical Transactions*, that both Hook and Halley had preceded Newton by ten or twelve years in starting the identical theory, though neither of them produced a *Principia* in order to establish it on mathematical principles.* That is, of course, Newton's great merit; just as the natural-selection explanations of Mr. Darwin are his. I ought, perhaps, to add that even Kepler is said to have also had some idea of the same kind as Newton, as to the influence of the sun in regulating the motions of the planets; but in truth Kepler's idea was not the same. He considered the sun had merely a directive influence, and not a force of attraction, as is explained in Whewell's *History of the Inductive Sciences*. (Vol. ii. p. 19.) In that admirable volume we are also told of the remarkable manner in which the *Principia* of Newton was looked and longed for, and how it was at once accepted whenever it was published. How some believed in the theory, even before the book came out—just as some now do in Darwinism, while yet only expecting Mr. Darwin's coming treatise, which is to make all clear! and how some—including even the acute philosopher Locke—believed in universal gravitation after the *Principia* was published, while acknowledging that they could not follow the steps of the reasoning by which it was mathematically established. I think it is very probable that

* *Journ. of Trans. of Vict. Inst.*, vol. i., p. 414.

something like this will also be the case when Mr. Darwin's *magnum opus* makes its appearance.

But these analogies as to the history merely of these two theories, however close, are of less consequence than the analogy that obtains respecting the groundwork and basis of the theories. "Is gravitation [Mr. Warington asks] a *real* cause capable by its action of controlling planetary motion? *i. e.*, is the hypothesis possible?" And so, he also asks, "Is Mr. Darwin's hypothesis *possible*?—Are the elements involved in it real elements capable of producing the *kind* of effects he ascribes to them?" I am sure, he will see, that I am giving his argument every possible advantage in thus keeping it constantly in juxtaposition with his chosen instance and the most popular science of modern times. And I will admit that just as we all know that a stone or an apple falls to the ground by its weight, and that therefore, so far, "gravitation is a *real* cause;" so we are all positively quite aware that "*the kind* of effects" Mr. Darwin lays stress upon, are certainly produced by climate, use and disuse, by growth with reproduction and inheritance, and by the external conditions of life and the consequent struggles for existence among plants and animals. I never heard of a man that denied an apple would fall to the ground; and I cannot conceive how those who believe in the unity of the human species can possibly deny against the evidence of their own eyes, that mankind at least have diverged and developed marvellously in all directions away from the original type of Adam and Eve, whatever we may consider their type to have been. But it is one thing to admit that an apple falls, and another to conclude that the moon, which does not fall, is under the same influence. So, it is one thing to admit that all mankind have descended from a common stock, and quite another *therefore* to conclude that man has descended from the same common stock as goats and monkeys. But, now, it is here that the analogy halts. Granted the first and second laws of motion, as propounded by Stevinus and accepted in the *Principia*, and granted that gravitation is a constant force; it is perfectly possible—and I think perfectly easy—to demonstrate whether or not a gravitating body could revolve round a centre of attraction without ever falling—that is, to prove or disprove the *possibility* of gravitation as a real cause capable of controlling planetary motion;—but I am not aware of any attempt to do this by Sir Isaac Newton or any of his followers. I say the possibility of universal gravitation might thus be tested by mathematical demonstration; but I do not in the least see how Darwinism ever can be. It would be unreasonable to require that *it* should be

established by such a test,—as unreasonable, I humbly think, as it was in the other case to dispense with such a test.

I therefore pass over the test of possibility as applied to Darwinism, to apply the other tests of harmoniousness, consistency and adequacy. And again, I must revert to the analogy of what Mr. Warington thinks established the adequacy of gravitation,—the discovery of the planet *Neptune*,—and which I will venture to say is strictly analogous to what was supposed to be the discovery of “the missing link between man and apes” in the famous Neanderthal skull, appealed to so confidently in the *Antiquity of Man* by Sir Charles Lyell, and in *Man's Place in Nature* by Professor Huxley. Again, I think the analogy will be found to run admirably on all fours. I am glad to follow Mr. Warington in his chosen analogies, and I am doing my best to complete them in thorough detail. Mr. Warington appears to have taken his view of the discovery of *Neptune* from Sir John Herschel's *Outlines of Astronomy*. But he ought to know that Messrs. Peirce and Gould, the American astronomers, have written also on the subject. From Mr. Gould's *Report on the History of the Discovery*, published in Washington in 1850, it appears that the tables used for the computations of the places of *Uranus* were calculated by M. Bouvard in 1821, and are now known not to represent the places of that planet, which was observed twenty times between 1690 and 1771, but was then mistaken for a fixed star. I cannot, however, here pursue the whole history of the discovery of *Neptune*. It is enough to say that certain irregularities or perturbations in the observed motions of *Uranus* led to the idea (which was shared by M. Bouvard himself) that these were caused by the influence of some exterior planet. Without going into the question of priority of discovery between Mr. Adams and M. Le Verrier, I shall here give you their respective computations of the mass, eccentricity, mean distance, period of revolution, and longitude of perihelion, of the supposed exterior planet, in a tabular form, alongside the figures deduced by Messrs. Walker and Peirce from actual observation of the planet *Neptune* after it was discovered. Thus:—

	THEORETICAL.		ACTUAL.
	ADAMS.	LE VERRIER.	WALKER AND PEIRCE.
Mass of Neptune	1 6,666	1 9,322	1 19,840
Eccentricity	0.12062	0.10761	0.00872
Mean distance from Sun	37.247	36.154	30.037
Period of revolution	—	217.378 yrs.	164.618 yrs.
Longitude of perihelion ...	299°.2	284°.7	47°.2

The mass, it will be observed, of M. Le Verrier, is more than twice, that of Mr. Adams nearly three times, the true one. The planet's actual distance falls short of its theoretical distance by about 500 millions of miles; its period of revolution is fifty years shorter; its eccentricity is only one-twelfth of the theoretical planet; and its longitude of perihelion in 1847 was only 47° , instead of 285° or 299° . The discrepancy as to the planet's heliocentric longitude I do not go into, as it would occupy too much time; and I think I have shown enough (all of which is probably new to Mr. Warington) to prove to him and all present, that the discovery of *Neptune* is not such a perfect confirmation of the certainty of the Newtonian hypothesis as he believes.* I must entirely object to bolstering up one theory in science by credulous appeals to other sciences, without investigation. It reminds me forcibly of the way in which idol-worship, that grossest of human absurdities, was maintained in its day, as described by the prophet Isaiah.†

But I must do Mr. Warington the justice to say, that in appealing to astronomy he only follows in the wake of Mr. Darwin himself, and of Professor Huxley and Dr. Büchner. But I doubt whether any of those Darwinians who thus make appeals to astronomy have paid much attention to that science. I am sure Mr. Warington is too candid not to make a frank admission, or to put me right, on this point as regards himself. But he must forgive me, if I am wrong; for I think I have good reason to come to this conclusion, when I find him saying in his paper, that "it would be impossible and absurd to discuss the motions of the fixed stars with the definition given that the fixed stars are those which never move," as if he were unaware that it is precisely on that assumption that the theory of "solar motion in space" was propounded by the first Herschel, and till recently had been the conclusion come to by all astronomers.‡ But Mr. Warington goes boldly beyond most people in his mode of "sticking up," if I may so say, for the astronomy of the day. Even if *Neptune* had not been discovered, his faith would not have been shaken, however perturbed the planet *Uranus* might be. He is quite prepared to assume that the perturbations might be caused by some invisible body; and, of course, upon that hypothesis, the planets may move as erratically as they please, and we may always have an invisible, but quite conceivable cause, to explain the whole matter! Upon this system of theorizing, it is quite

* Vide *Discovery of the Planet Neptune*. By J. Von Gumpach; *in loc.*

† Is. xli. 7. ‡ Airy's *Lectures on Astr.*, 4th ed., p. 173. Vide, also, *Journ. of Trans. of Vict. Inst.*, vol. i., p. 27.

ridiculous to take the trouble to discover new planets! But surely this is proving or assuming too much; and certainly, if we may reason thus, the discovery of *Neptune* was supererogatory! Apparently, Mr. Warington is not aware that there have been other *hitches* about gravitation; and that M. Le Verrier some time ago, in order to keep the solar system in gear upon the Newtonian hypothesis, was obliged to have recourse to this same mode of proof, and to invent an *invisible* "ring of asteroids between the sun and Mercury, the aggregate mass of which was comparable to that of Mercury; and another ring of asteroids near the earth equal to a tenth of the earth's mass," &c. I quote this from Mr. Hind's letter to *The Times* of 17th September, 1863. And I must further remind Mr. Warington of another discovery, made by our own astronomer, Mr. Adams, namely, that his predecessors had all omitted, in computing "the acceleration of the moon's mean motion," to allow for the effect of the sun's disturbing force when acting in the direction of a tangent to the moon's orbit. An account of this is given in Lord Wrottesley's address, as President of the British Association at Oxford, in June, 1860. On this point there were three great mathematicians, Adams, Airy, and the late Sir John Lubbock, on one side, with three equally distinguished names, MM. Plana, Pontécoulant, and Hansen, on the other; and strangely enough it is admitted by the English mathematicians, and by Lord Wrottesley, while they declare Mr. Adams to be right, that all the calculations come out more accurately when the sun's influence upon the moon is omitted, which it certainly ought not to have been, if the moon is subject to the sun's attraction! *

It is, however, notwithstanding such facts as these, that Mr. Warington makes his appeal to universal gravitation; and that Mr. Darwin says, "there is grandeur in this view of life with its several powers, having been originally breathed by the Creator into a few forms or *into one*; and that, whilst this planet has gone cycling on according to the fixed law of gravity, from so simple a beginning, endless forms, most beautiful and most wonderful, have been, and *are being* evolved." † And so, Professor Huxley, in *Man's Place in Nature*, is "fully convinced that, if not precisely true, Mr. Darwin's hypothesis is as near an approximation to the truth, as, for instance, the Copernican hypothesis was to the true theory of the planetary motions." Lastly, Dr. Büchner, as a frankly avowed atheist, gives us this extraordinary opening to his chapter on *Primeval Generation*:—"There was a time when the earth—a fiery globe

* Vide *Current Phys. Astr.*, in loc. (Hardwicke.) † *Orig. of Species*, p. 525.

—was not merely incapable of producing living beings, but was hostile to the existence of vegetable and animal organisms.” But afterwards, “with the appearance of water,” he tells us, “organic life developed itself”! * Then at Nottingham last year, in Mr. Grove’s address, while we had much the same sentiments repeated as to “the self-evolving powers of nature,” and the doctrine of continuity, we had actually gravitation questioned, although Mr. Warington has once more made this appeal to the discovery of *Neptune* as proving the truth of the theory, I very much fear without going into the merits of that discovery. And just so was a confident appeal made by Sir Charles Lyell and Professor Huxley to the discovery of the Neanderthal skull, as an evidence that there probably was some low-caste, half-human creature, intermediate between man and apes (which, of course, there might have been without proving transmutation from the one into the other); but upon investigation by Dr. Barnard Davis, it was found that the Neanderthal skull proved nothing, being evidently an abnormal development, caused by synostosis or ossification of the sutures, and that similar skulls, known to be the skulls of modern men, are in our museums.

Dismissing, then, Mr. Warington’s chosen analogy as worthless, I come to his direct arguments in favour of Darwinism. Mr. Warington, I think, very fairly states one of the main issues thus:—“That species grow and reproduce, and that they pass on their characteristics by inheritance, and that they are liable to variation is admitted by every one. The point at issue is whether they can so pass on and accumulate their ‘variations’ by inheritance as in the end to bring about specific differences,” *i.e.* new species. Of course it is obvious that, in order to settle this point, we must have a definite meaning for the word “species.” Well, Sir, I think I can furnish a meaning that, although somewhat absolute, will not be questioned, at least by Mr. Warington, namely this:—“The only fair definition of a species is a race of living beings possessing common characteristic differences from all others, which differences *at the present time are constant and inherent.*” This definition is Mr. Warington’s own! It occurs just before the other quotation I have made from his paper. It is admitted that at the present time the characteristics of species are constant and inherent. Yet, according to the same authority, if species are liable to such variation as may accumulate and in the end bring about new species, then Darwinism is to be pronounced “possible”! But, as

* *Force and Matter*, p. 63. (Trübner & Co.)

we have already seen, that is *not* the case "at the present time." It is here that I find Darwinism *inharmonious* with itself, with truth and nature. Mr. Warington very properly asks, "Is the method in which Darwinism asserts species to have originated one which there is reason to regard as in accordance with the ordinary and known workings of God?" And he adds, "it is here we come to the theology of Darwinism." "Its relations to Scripture," however, "he purposely passes by, because he does not believe that Scripture was ever meant to teach us science." I also pass by the teaching of Scripture at present, not because I can admit it has not revealed to us a knowledge of the creation, but that I may meet Mr. Warington on his own and the lowest ground. He says, "In the first place, Darwinism assumes no cause, force or influence other than those *known* to be at work at the present day." And yet he has also said that, "at the present time," the characteristics of species are "constant and inherent." Well, Sir, I call that *inharmonious*. But he goes on, and speaks for others besides himself. He says, "We believe that all living things we now see about us were made by God, by means and under the influence of these causes involved in Darwinism;" nay, he says (and I am sure it must have astonished almost all who heard him): "We feel no difficulty in so believing;" and he then asks triumphantly, and (granted his assumptions) with admirable logic, "Why, then, should we feel difficulty in so believing as to all living things in the past?" I suppose I must astonish him in turn, if my answer is, That we do *not* believe in the Darwinism *of the past*, which he seeks to establish, because we do *not* believe, as he assumed, in the Darwinism *of the present*. We do not believe—though he told us we did—that God made all living things we now see about us by means of causes involved in Darwinism. Mr. Warington seems to think he proves this because we acknowledge God to be our Maker; and he has previously used similar language in this Institute, which was not then answered. Let me now say, then, that in discussing "Creation" philosophically, it cannot be admitted that *we* and all living beings we *now* see around us were "created" at all. There is a true sense in which we are all regarded as the creatures of God, and as therefore created by Him; but that language is inapplicable in philosophical discussion, in which we must be regarded as having been born by ordinary generation, and *not* "created." But as far as causes or influences are "known to be at work at the present time," man has always produced man, and animals always animals, "after their kind." We *know* nothing of

transmutation of species, and we therefore must reject this theory as not in harmony with what we do know of nature.

But it may be retorted that I have admitted that we do possess a knowledge of the "*kind* of effects Mr. Darwin lays stress upon"—such as the influence of climate, use and disuse, and external conditions generally, upon plants and animals; and if so, why not admit his whole theory? But I reply, it is not for us to go beyond our knowledge, or to make nature itself what we might call "harmonious." Our duty is to have our hypothesis in harmony with nature, such as it is. I admit these effects, but only within the limits of nature's laws, and according to what we know. I must exclude from my definition—again using Mr. Warington's words—"all mere transient sports, or temporary variations," as well as "all apparent varieties dependent upon situation, climate, &c." Holding that *exceptio probat regulam*, I reject a theory which turns exceptions into rule, and reverses those laws of nature which are known to be "constant and inherent at the present time." To have recourse to an analogy suggested by Mr. Warington's test of harmoniousness, we know that an occasional and delicate note of discord may even serve to increase the sweetest harmony; but were discords to become predominant in musical composition, all harmony would be destroyed. And so with the constant discords Darwinism seeks to make the rule of nature. They are utterly destructive of harmony.

Besides, let it be granted that varieties may become confirmed in their differences, and thereby become new species, does it then follow—as Mr. Wallace and others have argued elsewhere*—that therefore this process might go on *ad infinitum*, and new genera be also developed from species? Certainly not. You may call this granting the first step in the process, and therefore say I must grant the whole. But, I ask, will Mr. Warington, then, admit the same kind of argument as regards the first steps of his reasoning? Can he, for instance, or does Mr. Darwin in fact, attempt to get a beginning for the first few forms of life, or for the "one" to which analogy would lead him, without a breathing of life by the Creator into that first one, or into these few first forms? No. And, if not;—if you must have the Creator to give you your first form or forms of life, why limit Him to these? Why not begin with more than this one or meagre few? Why should He not have given life to "every living creature after its kind," *i.e.*, to every *genus* at least, or even to many

* *Anthrop. Rev.*; vol. ii., pp. cxxviii, cxxix.

primitive original species ? But, if not ;—if you will not grant this, then be logical, and make your own theory utterly “harmonious,” as the more outspoken Darwinists do. These may be Mr. Warington’s deductions from Mr. Darwin’s book, or Mr. Darwin’s own views ;—but hear what Dr. Louis Büchner says :—

The law of analogies ; the formation of prototypes ; the necessary dependence upon external circumstances which organic bodies exhibit in their origin and form ; the gradual development of higher organic forms from lower organisms ; the circumstance that the origin of organic beings was not a momentary process, but continued through all geological periods ; that each period is characterised by creatures peculiar to it, of which some individuals only are continued in the next period ;—all these relations rest upon incontrovertible facts, and are perfectly irreconcilable with the idea of a personal almighty creative power, which could not have adopted such a slow and gradual labour, and have rendered itself dependent upon the natural phases of the development of the earth. (pp. 84, 85.)

He goes on in another passage, in which he quotes Linnæus, just as Mr. Darwin does :—

The work of nature, with its half-accidental, half-necessary products, has, on the contrary, been infinitely slow, gradual, and not premeditated. We nowhere perceive in this work an origin indicative of a personal will. “Nature,” said Linnæus, “performs nothing *per saltum* ;” and, indeed, every new discovery in natural history confirms this axiom. The plant passes imperceptibly into the animal, the animal into man. All endeavours to fix the limits between vegetable and animal life have hitherto failed ; nor is there any existing insurmountable barrier between man and animal, of which we hear so much. (p. 85.)

This reasoning certainly makes Darwinism harmonious with itself ; but it also brings it into discord with nature and with even the conception of Deity.

But now I come to the inquiry, is Darwinism *consistent* ? Here Mr. Warington rests as a kind of proof upon what Lord Bacon has pointed out as being the very A B C of theorizing. Mr. Warington thinks it the severest possible test to require that a theory should apparently agree with the facts or phenomena it has been invented expressly to account for. Why, of course, it must do so, more or less, or how could any sane man have either invented it, or others entertain it for a moment ? And certainly, of all the theories ever propounded by man, Mr. Darwin’s is the most consistently inconsistent and most variously adapted so as to account for almost everything.

Naturalists are all at sea, it seems, as to what are varieties and species, or even, as to how orders and sub-orders are to be distinguished. But surely this is the exception and not the rule; and when they know better, and can divide more scientifically, this overlapping and confusion, upon which Darwinism wishes to found itself, would be got rid of. Mr. Warington himself admits that "in the majority of cases there is no such difficulty, the specific differences being clearly marked," though he tells us the intermediate varieties of brambles have sorely puzzled him. The gradations are sometimes so fine, that is, varieties are so very much alike, that they shade off into one another; and this, it is argued, is just what Darwinism would have expected. Very good, let us grant so much. But how then can we also grant, that when differences vary exceedingly—that is, when species or varieties are not at all alike—that this also should be just what Darwinism wants in order to prove it? I call that an inconsistency, which Darwinism can only reconcile, because in itself a conglomeration of inconsistent principles.

But I go on. If Darwinism be true, there must be "an enormous number of intermediate forms." And, of course, so there are; precisely what Darwinism would lead us to expect. But at the same time the geological record does not prove the continuity or universality of these gradations; but what of that? the theory does not want them. On the contrary, "We may safely assert [Mr. Warington says] that the geological formations now being produced could only most exceptionally give any indication of the truth of Mr. Darwin's hypothesis, supposing that hypothesis to be true." So, it seems that Mr. Baden Powell's, Mr. Darwin's and Sir C. Lyell's laments over "the imperfection of the geological record" are all a mistake; according to Mr. Warington, the theory can dispense with such evidence. It is equally to be regarded as true, whether we find that intermediate forms existed or not. Then Mr. Warington pertinently asks, "What geological evidence *would* satisfy an anti-Darwinian?" And I venture as frankly to say, *not any evidence of this kind whatever*. No want of it, as we have seen, disturbs Mr. Warington's faith in the theory. No amount of it could, we may be sure, ever convince any one whose objections to Darwinism are worthy of consideration. As regards geological evidence—or the *want of it*—"the foregone conclusion (I fear) would colour everything"!

It is in this part of Mr. Warington's argument that we come to a tell-tale expression, which I do regret to discover. In his view, the peculiar variations to be found in the different genera of the *Connaraceæ* do not present to his mind such a "symmetry

and manifest method" as to be "suggestive of especial design and [what he calls] arbitrary plan." *Design* has usually been regarded as proving the overruling of Divine intelligence and wisdom in nature. According to Mr. Warington, it merely means *arbitrariness*. Even Büchner has a better idea of what he, as an avowed atheist, openly opposes. He says, "Design in nature has ever been, and is still, one of the chief arguments in favour of the theory which ascribes the origin and preservation of the world to a ruling and *organizing* creative power." (p. 89.) You must pardon me going on, and quoting some passages that will grate upon your ears:—

Is it not more natural [he asks] to consider certain phenomena as the effect of changes in the temperature, than to imagine a heavenly tailor who takes care of the summer and winter wardrobes of the various animals? The stag was not endowed with long legs to enable him to run fast, but he runs fast because his legs are long. He might have become a very courageous animal, instead of a timid one, had his legs been unfit for running. The mole has short spatulated feet for digging; had they been different, it would have never occurred to him to dig. Things are just as they are, and we should not have found them less full of design, had they been different. (p. 91.)

He then quotes Mr. Darwin, and especially refers to his view of the development of the eye, so admirably handled in our Vice-President's *Inaugural Address* last year; and then adds—reminding us how very old this pretentious Darwinism is:—

Empedocles, the Greek philosopher, already taught that, when matter assumed shape, there were many irregular forms which could only partly sustain themselves, and which only slowly attained forms adapted to certain ends. (p. 92.)

According to Büchner, nature is "guilty of many purposeless absurdities" (p. 94); and he says that comparative anatomy "makes us acquainted with a number of physical characters which are perfectly useless to the animal possessing them, and which appear merely as the rudiments of an organ which in another species is more developed, and consequently useful to the animal." (p. 97.) Again: "Contrivances apparently purposeless are numerous in the structure of animals and plants." (*Ib.*)

And yet, in some of his statements, he is more moderate than Mr. Warington. For instance, Mr. Warington considers it indisputable that "*all* living beings reproduce themselves in a geometrical ratio of increase, which must inevitably lead to an overcrowding, a jostling, a struggle, both for position

and subsistence." Büchner more cautiously says:—"The fruitfulness of *many* animals is so great that, abandoned to themselves, they would in a few years fill up the seas and cover the earth." (p. 98.) Before I pass on I must also notice that, according to Büchner, one of the most important facts against the theory that "nature acts with conscious design, is the production of monstrosities." (p. 98.)

We hear a good deal of persons "not understanding Darwinism." How admirably, on the other hand, do the Darwinians appear to understand what they oppose. According to Mr. Warington, "especial design" means "arbitrary plan;" according to Büchner, "our argument from design" must imply that "nature acts with *conscious* design," as if "nature" were our Deity!

Before I proceed to consider what Mr. Warington calls the *adequacy* of Darwinism, I must notice the paradoxical consistency of the very name of the theory. Not long ago in this Institute, when discussing the subject of *Miracles*, we had a definition of nature put forward (and I think at least tacitly accepted by Mr. Warington in his argument), namely, that the word has only a meaning with reference to a settled course or order, or law, implying a lawgiver; and then "the uniformity of nature" was constantly in Mr. Warington's mouth. Well, I think we would all admit—unless we had a foregone conclusion to colour our judgment—that the word *selection* implies choice and an intelligent selector. But Mr. Darwin's theory is well named "the law of natural selection"—natural being used in antithesis to what is according to law or to uniformity, and selection as opposed to either choice or design. The whole thing means only "law" *per accidens*—that is, lawlessness; and, instead of "natural selection," we really know it is a theoretical process of *accidental existence and extinction*; a jostling scramble and struggle for life; a *saute qui peut* in creation; with Providence, when not consistently set aside, exercising only the prerogative of the heathen fate, and ruling mercilessly *Vae victis!*

But still we are gravely asked, "Are the causes alleged *sufficient* to account for all the specific differences known to exist?" We are very fairly told in advance, that it is very far from satisfying the hypothesis merely to admit that *some* races may have originated as Mr. Darwin thinks; the proposition being "that *all* have." But here Mr. Warington has betrayed himself, and his frank mode of putting it is apt to betray us into a false and illogical position. Consistently inconsistent once more, the reasoning plays with words, like the demented Prince of Denmark:—"all" does not here mean all;

it means only "all *but one*," or as Mr. Warington himself prefers—less consistent than even the distracted Hamlet!—*all* means all living beings, *excepting some eight or ten* progenitors; and thus going *per saltum*, and I fear unphilosophically as well as illogically, from the very condition precedent he had laid down,—namely, *all* or nothing,—he proceeds to his second inquiry as to "adequacy." In fact, you will find that now the theory does not "run on all fours" to any purpose, or even with itself. It really does not account for the *origin* of species at all! It asks you first to give it four or five progenitors for animals and four or five for plants, and then it can go ahead. The theory is "possible," in Mr. Warington's opinion, if you will merely grant that "species vary," and that their variations "*frequently* have a bearing on their adaptation to the circumstances of their life," &c. To which I reply, this is excellent reasoning to account for *new* varieties, or let me again concede for perhaps *new* species; but how does it account for the *origin* of species? It might account for "*some* races," and "*some* specific differences"; but that "is very far from satisfying the hypothesis, which is not that *some* races have thus originated, but that *all* have."

This is Mr. Warington's own refutation of his own argument. But this argument had been preceded by other *obiter dicta* equally self-contradictory. For instance, this:—"There is a perpetual struggle for existence going on, both among rival races and rival individuals; and this struggle must lead to *selection*." But then this so-called selection merely follows the struggle among the rival races and rival individuals that are presupposed to exist. It does not account for their origin. And *before* we get into this crowd of races and rivals, even an "unprotected female" might have been safe, and not forced to make struggles for life! Surely the four or five progenitors at most of plants and of animals would not, on the face of this wide, wide world, have felt themselves subject to overcrowding and jostling and struggling, either for position or subsistence!

But Mr. Warington, who has made up his mind to the long geological periods, though he objects—I think very properly—to the geologists' special and detached creations, quite omitted to tell us whether the four or five plants of Mr. Darwin's theory were specially first created, and if so, how long it was after them that the four or five animals were next also specially created; or if they were all specially created together? And this is no idle question, intended merely to puzzle a Darwinian to say what he really finds intelligible in the hypothesis he submits to us as credible. For, let me ask this further ques-

tion, with reference to the fertilization of orchids, How could they possibly have been fertilized and continued in being—supposing we pass over the difficulty of their first coming into existence—without the co-existence of the insects required for their propagation, according to Mr. Darwin's interesting volume on the subject? I may remind Mr. Warington that there is, if I may use the term, a theory of creation—not that of special creations invented by geologists with long gaps between—but an account of continuous creation, in which the insects that fulfil this purpose of nature come quickly into being by the Creator's word, very shortly after the orchids themselves, with all the original flora of the earth, burst forth into existence in all their marvellously varied beauties and blossoms.

And here I must observe, with some satisfaction, that throughout Mr. Warington's paper, he never ventures to propound a difficulty as regards that view, or to draw a contrast between Darwinism and that Divine theory of continuous special creations completed within six days; for he only contrasts the humanly invented theory of special creations by fits and starts, with ages intervening, and the gradual development theory of Mr. Darwin, which he prefers.

And now, Sir, I think I might claim to have met fairly all Mr. Warington's leading arguments, and proved Darwinism to be inharmonious, inconsistent, inadequate, and therefore irrational and incredible. But I am content to meet it on still lower ground; not to press principles too logically against it; to allow it its illogical beginnings, and to leave the highest ground, in order, as it has been characterized, "to fight the battle in a bog," where the struggle for existence is already imagined to be going on; to grant so far, as Mr. Warington asks us, the "possibility" of the theory, and test its adequacy upon points of detail.

And here I must quote for distinctness what our author calls the elements of the theory:—"1. Growth with reproduction; 2. Inheritance which [I agree with him] is almost implied in reproduction." And these two definitions, in my opinion, might fairly be merged into one we have all often heard, that "like produces like," which is implied by either "reproduction" or "inheritance." Then we come to No. 3, which is, "Variability, from the indirect and direct action of the external conditions of life and from use and disuse; and 4th, a ratio of increase so high as to lead to a struggle for life." Now No. 3, you will observe, is in antithesis to Nos. 1 and 2. *Variability*, and not reproduction or inheritance, is what it predicates: in other words, it requires us to hold that "like does not produce like" in nature. It is here we have *the* essential

element of Darwinism. The whole question is,—How far this is true? Is *like producing like* the rule in nature? or, Is variation the rule, and reproduction and inheritance exceptions? This is the grand issue we have to settle. Now I have said that Darwinism converts the exceptions into the rule; and so does Mr. Warington in the conclusion he draws from these conflicting “elements of the theory.” He says,—“The result is natural selection, *entailing divergence* of character and the extinction of less improved forms,”—in other words, the result is (1) *Dissimilarity*; and (2), in so far as there is *not* dissimilarity, *destruction*, or, euphemistically, “*the extinction of less improved forms.*”

I must here observe, that the effect of “use and disuse,” which is really the leading principle of the theory of Lamarck, is stuck into Mr. Warington’s third definition, (following, however, in this his master,) because Mr. Darwin’s own peculiar theory of “the struggle for existence” is itself felt to be inadequate. Here is another and fuller account of Mr. Darwin’s reasoning, which I put forward in a paper read before the Anthropological Society three years ago:—

As regards vegetable life, Mr. Darwin dwells almost exclusively upon his law of natural selection proper, to account for modifications. But, when he comes to speak of animals, he recognises that “the external conditions of life, as climate, food, &c., seem to have induced some slight modifications.” He also says, that “*habit*, in producing constitutional differences, and *use* in strengthening, and *disuse* in weakening and diminishing organs, seem to have been more potent in their effects.” When, however, neither *use* nor *disuse* appears to operate sufficiently to justify Lamarck’s theory, then Mr. Darwin is ready to draw attention to “the most important consideration, that the chief part of the organisation of every being is simply due to *inheritance*,” and so he accounts [as any anti-Darwinian would do] for the webbed feet of the Upland goose “remaining unchanged;” and he curiously describes them as being “rudimentary in function, though not in structure!” (*Orig. of Species*, pp. 185, 204, 219.) In fact, Mr. Darwin confesses that he is “well aware that scarcely a single point is discussed in his volume on which facts cannot be adduced, often apparently leading to conclusions directly opposite to those at which he has arrived.” (p. 2.) Yet he very ingeniously claims all these conflicting facts as illustrations of one or other of the various theories, old and new, which he has selected to form into one, of a very plastic character indeed, itself a practical specimen of “transmutation from varieties.”*

Now it must be perfectly plain, I think, that I do not strain the Darwinian hypothesis unfairly, when I say it makes variation

* On Anthropological Desiderata.—*Anthrop. Rev.*, vol. ii. p. cxx.

and divergence the rule, and almost, if not altogether, sets aside what Mr. Warington calls its "two first elements," and I the canon that "like produces like." For, if not;—if that were the rule, then if we begin with one form only, only one form would have been reproduced; or, if we begin with a few forms, or with eight or ten, then only the few forms or the eight or ten, instead of myriads, would have been the result. If, on the other hand, it is attempted to turn this logic against the hypothesis that like producing like is the rule of nature, and variations are the exceptions; and if I am told that I cannot account for those myriad forms which do vary before our very eyes, as I frankly admit they do,—I beg leave to reply, *non constat*. The theory of Creation I contend for, assumes that by the fiat of an all-wise and omnipotent Creator, the earth, made up of varied elements, brought forth a varied flora of several kinds to begin with; and afterwards that the waters and the earth likewise brought forth every living creature after its kind. It begins with varied genera and species, which are to increase and multiply in the earth and waters; *like producing like, "after their kind," and variations producing new varieties.*

At the first, in any one genus or species, it does not begin with one merely, according to *our* hypothesis, but always with two at least—"male and female created He them,"—and these pairs are never precisely alike. Hence the consistent origin of fresh varieties upon this hypothesis,—it may even be of *new* species. What is common and like in the two parents or progenitors, we may believe to be naturally inherited and reproduced; wherein they differ or vary, the result will be a fresh difference or modified variation. If Mr. Warington's woman with the web-foot had only had—like the Upland goose—a web-footed mate, this *lusus nature* might probably have been perpetuated, instead of fading away as it did, "a mere transient sport," obliterated in a few generations.

Not to follow in detail the other instances he has given of abnormalities and defects, transmitted exceptionally and afterwards extinguished, I come to his summary of what they teach us. He admits that in these instances the varieties were highly disadvantageous or of even an abortive character, and not *improvements* upon the ordinary forms of life. But what of that? You have only to suppose the contrary to the facts of the case, and all will go well with Darwinism. He says, "Suppose these variations *had* been beneficial!" I reply, *They were not*. But I must quote his *naïve* argument at length:—

Had the variations been beneficial, and so themselves have tended to

preservation—had, for example, the palmation of the toes occurred in a bird living partly in the water, or the baldness in another to whom head-feathers were inconvenient (and the like phenomenon has been observed to be hereditary in doves) ; or, again, had similar changes taken place, only in an opposite direction—say the strengthening of the lungs instead of their weakening, or the addition of pigment to eyes formerly devoid of it, instead of its withdrawal from eyes formerly possessed of it ; had, especially, owing to the favourable influence of such variations, and the consequent multiplication of their possession, some of the successive generations been born of parents both of whom varied in the same manner ;—*had this been so*, we cannot doubt but that races of living beings would have come into existence differing most markedly in structure from their progenitors, and forming species which the anti-Darwinian naturalist would ridicule the idea of ever having sprung from the source they did.

Of course, if Mr. Warington may be allowed to vary the facts of nature as he pleases, and also to select them, as well as to vary his arguments irrespective of his own premises and logic, there can be no doubt he may establish Darwinism or any other fanciful hypothesis.

But now I must pass rapidly on, and notice a few points only, to show that I have not overlooked them, though I cannot now possibly notice all. As to “use and disuse,” the Upland goose alone refutes Mr. Darwin and Lamarck. As to Mr. Warington’s difficulty with respect to Pliny’s evergreen plane-tree, it is explained, I think, in a word:—“The earth brings forth,” as God commanded ; and if the appropriate soil is wanting for what has been once produced, no doubt a species or variety of plant may die out or be greatly modified. This also, I think, affords the simple explanation why a heavier crop of hay is obtained from mixed seed than from seed of a single kind ; and it teaches why the rotation of crops in farming is beneficial. It also refutes the endless prolificacy theory of individual forms. They would soon exhaust the soil that suits them, and then die.

As regards all Mr. Warington’s instances of sailors’ long sight and students’ short sight, of right-hand use and long-legged runners, down even to the aldermanic development of the stomach, he surely knows that no long-sight or short-sight race has been thus produced ; that throughout the world all races are generally right-handed ; and I don’t believe he can prove that all the swiftest runners have the longest legs ; while it is notorious that all the feasts of the Corporation of London have not served to produce such a pot-bellied race as the miserable, half-starved Bushmen in South Africa !

I grant, freely, that there are variations of the *kind* Mr. Darwin appeals to. I deny that such variations are either in

the direction or to the extent he wishes us to believe, contrary to every instance he himself has adduced. His analogy of *artificial* selection by man in the breeding of pigeons, &c., is only another of his illogical efforts that even his own facts refute. For we know that all artificial breeds of pigeons or rabbits become very soon extinguished by reversion to their common type, when left to themselves and to *nature*.

Mr. Warington tries to obliterate the peculiarities we know as regards species, although in another place he admits specific differences at the present time to be constant and inherent. And as regards his belief in new species being developed progressively and upwards from lower to higher forms; because, perhaps, the lower forms, like those that now occupy the bottom of the ocean, are generally found embedded in strata below fishes that swim, and animals that live on the land;—I must quote from Professor Huxley's address to the Geological Society in 1862:—

Obviously [he says,] if the earliest fossiliferous rocks now known are coeval with the commencement of life, and if their contents give us any just conception of the nature and extent of the earliest fauna and flora, the insignificant amount of modification which can be demonstrated to have taken place in any group of animals or plants is quite incompatible with the hypothesis that all living forms are the results of a necessary process of progressive development, entirely comprised within the time represented by the fossiliferous rocks.

This, of course, I use only as an *argumentum ad hominem*. I have already said that no dead remains of formerly existing gradations in the fauna or flora of the world could prove that they developed upwards and out of one another, though I admit variation within nature's known limits. Here, again, however, Darwinism requires us to reverse the facts of nature. The author of the *Vestiges* thought that no fish existed at the period of the lower Silurian deposits, but only crustacea and molluscs. But remains of fish have since been found even below that formation, and not merely of fish of a low kind, but in the highest state of organization.

If we think, with Hugh Miller, that "There was a time when the ichthyic form constituted the highest form of life," still the sea during that period did not swarm with fish of the degraded type. At the time also when (he concludes) all the carnivora and herbivorous quadrupeds were represented by reptiles; still there are no such magnificent reptiles now, as then reigned on the earth. If again (like Miller) we think there was a time when birds alone represented all the warm-blooded animals of the globe; yet we find from the prints of

their feet left in sandstone, that the tallest man might have walked underneath their huge legs. So again, when we come to the higher strata in which quadrupedal mammals became imbedded by some convulsion of nature, what was their earliest character? We find the sagacious elephant, now extinct save in Africa and Asia,—and there restricted to two existing species,—we find it almost over all the old world, and a closely allied genus occupying its place in the new. “Most certainly all the geological facts (says Hugh Miller) are hostile to the Lamarckian conclusion,”—which Mr. Darwin has only rechaufféed and served up with some ingenious trimmings. “As if (continues the author of *The Testimony of the Rocks*) with the express intention of preventing so gross a mis-reading of the record, we find in at least two classes of animals—the fishes and reptiles—the higher races placed at the beginning.” To quote, with some modifications, from another writer:—Thus it is too with birds and quadrupeds. Where deepest down in the earth’s strata their remains appear, they show no evidence of just emerging from a lower order. They stand forth in full development, and usually of giant size, compared with such of the same orders as occupy a super-position. Indeed, the evidence of geology most naturally tends to the conclusion, that each of the successive races of creatures, found imbedded in the earth, was created in its highest state of perfection; and that the varieties of the same orders afterwards found, testify rather to a process of degradation than to a process of development towards a higher class.*

Finally—as regards the phenomena of embryology, and the marked similarity in all organic development, and the existence of what are called “rudimentary organs,” occasionally not developed,—they appear to me only to teach that all organic growth proceeds upon common vital principles and laws, which, the true theory of creation enables us to understand, must have been ordained by infinite Wisdom and with beneficent Design. To establish this, however, is not my present task; which has been only to endeavour to prove that Mr. Darwin’s theory, as advocated by Mr. Warington, is utterly *incredible*.

Captain FISHBOURNE.—I rise to speak on this subject, in order to look at it from a common-sense point of view, and to express my protest against Darwinism. Mr. Darwin and Mr. Warington have founded many of their arguments upon the effects of man’s interference with nature, as for instance in the case of domestic animals. The alterations, brought about by man’s

* Vide *Creation’s Testimony to its God*, 10th ed., p. 133.

intelligence, we must admit ; but these gentlemen seem to overlook that even the differences referred to are very limited, and that man's utmost skill fails to enlarge them beyond these limits. Moreover, the moment man's influence is withdrawn, the animals return to their original condition, clearly showing that the alterations thus effected were abnormal. That this is the case with pigeons is admitted both by Mr. Darwin and Mr. Warington, and several instances are given in illustration of the fact ; and yet, on the other hand, they argue as if the changes made had become inherent and constant. We know that this not so ; but, granting that the changes have become inherent, we are then involved in this difficulty, that there is not a "progress to perfection" according to the Darwinian theory, but a stopping short in these varieties which we are told are fixed. In either case, then, a violation of the theory. Mr. Warington states that these changes are brought about by "the law of natural selection," but of this there is no explanation—

Mr. WARINGTON.—If you read the paper you will find there is.

Captain FISHBOURNE.—I am aware of what is stated ; but I say there is nothing intelligible in what is called "natural selection." Are we to understand that the flower, that requires a *particular* fertilizing pollen to produce a given change, *selects* both the insect that is to carry the pollen as well as the particular pollen that is to be carried to it ? Or are we to suppose that the insect is the selector ? If neither is, then there is no selection. If the insect is, then it is required to exercise a degree of intelligence far transcending anything that can be conceived of in man. The fact is, there is no such thing in nature as this natural selection : it is contrary to common sense to suppose anything of the kind. As to the most difficult part of the theory, that of transmutation, we are left without even a hint of the process, and are given, instead, a lame attempt at the description of the formation of an eye. Mr. Darwin says :—

"It is scarcely possible to avoid comparing the eye to a telescope. We know that this instrument has been perfected by long-continued efforts of the highest human intellects, and we naturally infer that the eye has been formed by an analogous process. But may not this inference be presumptuous ? Have we any right to assume that the Creator works by intellectual powers like those of man ? If we must compare the eye to an optical instrument, we ought in imagination to take a thick layer of transparent tissue with spaces filled with fluid, and a nerve sensitive to light beneath, and suppose every part of the layer to be continually changing slowly in density, so as to separate into layers of different densities and thicknesses, placed at different distances from each other, and with the surfaces of each layer slowly changing in form. Further we must suppose that there is a power (*natural selection*) always intently watching each slight *accidental* alteration in the transparent layers, and carefully selecting each alteration, which under varied circumstances may in any way or in any degree tend to produce a distincter image. We must suppose each new state of the instrument to be multiplied by the million, and each to be preserved till a better be produced, and then the old ones to be destroyed." (p. 219, 4th edition.)

This is the idea given of an eye forming itself. But what determines the kind of eye that is to be formed—whether it is to be the eye of a cabbage or that of a man ; for by the theory they are equally derivable from the "one

primordial monad"? Reference has been made to sailors' long sight. Now it is notorious amongst us sailors, that sight to a great extent depends more upon the mind than upon the eye. If a lad at sea says he cannot see what others see, he is told he must be made to see, and he is punished for not seeing; and we find that he very soon learns to see. But can any intelligent person suppose the eye has been improved so as to produce this effect? Certainly not. It is well understood that it is the mind that has been exercised, and its perceptive faculties have been developed, while the eye has been unchanged. But the Darwinian theory supposes this power is in the eye. Are we then to consider that there is no mind; or that matter is all mind, or mind all matter? It really involves this:—If there is such intelligence exercised by material tissue, then is matter all mind! A living philosopher tells us that there is no matter, and that matter is only a condition of mind. This controversy ought to be settled, before we are asked to believe in such a theory as this.

Rev. J. MANNERS.—I have not had the pleasure of reading Mr. Warrington's paper, but have been much interested with that we have heard this evening by our Hon. Sec., Mr. Reddie. This subject, I must say, appears to me a very curious one. I recollect reading some time ago some verses in *Blackwood's Magazine* (for May, 1861) *apropos* to this; and though I do not quote them as an argument, they are much to the point. They begin:—

“Have you heard this strange theory the doctors among,
That all living things from a monad have sprung?
This thing hath been said, and now shall be sung;
Which nobody can deny.”

Then they go on to account for the formation of elephants, giraffes, &c. thus:—

“A very tall pig with a very long nose
Sent down a proboscis quite down to his toes,
And then by the name of elephant goes;
Which nobody can deny.

“A deer with a neck which was longer by half
Than most of its family (please not to laugh),
By stretching and stretching became a giraffe;
Which nobody can deny.

“Pouters, tumblers, and fantails are from the same source;
The racer and hack may be traced to one horse;
So men were developed from monkeys, of course;
Which nobody can deny.

“An ape with a pliable thumb and big brain,
When the gift of the gab he had managed to gain,
As a lord of creation establish'd his reign;
Which nobody can deny.”

Afterwards the author goes on to show how—

“Fleas, flies, and lobsters in order succeed,
And ichthyosauruses follow the lead.”

And I think, without the writer of these verses going into any deeper philosophy about the matter, there's a good deal of fitness in what he says in reference to "this strange theory the doctors among"—this Darwinism. But, to come to the subject in a somewhat graver manner, it is deeply important that we should consider the subject well ; because if for a moment I can imagine that man is merely an advance on a first-rate monkey—that I am to consider my origin no higher than a respectable ape, who sprang from a funnyish monad, myriads of myriads of ages ago—the probability is, if I don't take care, I may return to that condition, whatever it may be. (Laughter.) If we move in cycles of this kind, who can say this will not be the case ; for nothing rises higher than its proper source ? I am sure that no one here who would admit a theory like this—would doubt for an instant that it is possible, yea, probable, that we should come back to such—

The CHAIRMAN.—I rather think that you are in perfect accordance with Darwin, because he tells you that, do what you will with the pigeon, it will go back to the original type ; and therefore there is that probability as regards man. It is quite in accordance with the theory.

Rev. J. MANNERS.—Now, as to the truth about man. What is the true living, real, divine philosophy concerning man's nature and origin ? I am fully convinced this is truly found as recorded in Genesis ; and so the theory of Darwin may readily be cut up and shown to be absurd in the highest degree. Let us for a moment or two glance at the account there given :—1st. We have in the beginning that God created the heavens and the earth—that darkness was on the face of the deep—that God said, *Let there be light*,—then comes a separation between the light and the darkness—and, let the waters be gathered together into one place, and let the dry land appear. We have the manifestation of the sun, moon, and stars, which are for signs, seasons, days, years, to divide between the day and night. We read—*Let the waters bring forth abundantly the living thing after its kind—let fowl fly in the midst of the heaven of firmament—let the earth bring forth grass, and the living creature after his kind, &c.—and it was so—it is so*, according to this divine fiat. Hence we see that the inferior orders of the creation are living, moving, and acting according to their peculiar nature and in obedience to the law contained in this *fiat*, or "*Let be* ;" therefore we see this order of the creation rising no higher than the properties it received in its origin, in harmony with the divine will and purpose, and therefore we reasonably infer its probability. We must admit this, because we see how all things harmonize therewith. Let the earth, the waters, the sun and moon, do or act so and so ; and we see the manifestation of wisdom herein—that *all* do act in perfect and simple obedience to it, and exhibit the various powers, faculties, virtues, and properties of their "*nature*"—a nature which is very little understood, but which, when deeply investigated, will be found to subsist and operate in beautiful concord with the *Will* of the Creator. We now come to the creation of Adam. We do not find it stated, *Let the earth bring forth men and women* ; or, let it produce apes and monkeys, and terminate in man. No ; here is the grand distinction.

God said,—Let us make *Adam* (I prefer using that word) in our image and after our likeness ; and let them have dominion over all things, the fish of the sea, the fowl of the air, and over everything upon the earth. So God created Adam in His own image ; in the likeness of God created He him—male and female created He them ;—and observe, He called *their* name Adam (Gen. v.), in the day they were created. In this there is doubtless a glorious truth hidden : before the separation of *Eve*—before the deep sleep upon Adam—He called their name Adam, and blessed them ; and all was very good. Now, there can be no doubt that Adam, or man, heads-up the entire creation ; that every element of the universe is in him—fire, light, magnetism, darkness, &c. ; in fact, all the elements of the visible ; and for this plain reason, that he was to rule over all. We observe, too, that he is an out-birth of the *Eternal* : for God *breathed* into him of His own eternal being. He did not breathe the breath of life into animals and vegetables, but He did into our ancestor ; and hence the reason why man can never rest or be satisfied until he find his rest in the *Word* and Spirit and bosom of God. We notice, too, this fact, that *man* is fallen from his high estate ; so also is the world. Man, we say, is fallen from his paradisiacal state into this elementary world, which now brings forth its thorns, briars, and thistles. Our roses have thorns—the elements their storms, tempests, and discords ; the one *pure* element is divided into four ; and we witness great and seeming contrarieties and confusions. All this is very different to its primal state, when all was very good. This will solve many difficulties. (Hear, hear.) The results of the fall are everywhere apparent—specially we feel this in ourselves. Can any one say it is not so ? Does not the whole creation give utterance to this truth—that it is in bondage—that it is waiting to be delivered from its bondage of corruption, and to be brought into a liberty which it once enjoyed ? The Scriptures tell us it shall be accomplished ; that the creation was made subject to vanity not willingly (Rom. viii.), and this for a period, and that it shall be raised into the glorious liberty of the children of God. I wish for a few minutes, however, to refer to *our own gradual* development from one state to another ; from one of low to one of high degree ; from an earthly to a heavenly. Whence these aspirations ? I see two men very different in their motives, actions, and desires ; one acting according to pride, ambition, covetousness, envy, and the like, selfish in the extreme, whose views seem to be bounded by time's limited horizon ; and I see another, whose every desire and motive is to reach and realize eternal things, passing by the temporal, almost, to enter into the everlasting ; whose being and walk seem wholly centred in communion and fellowship with God ; who knows that he is a *changed* man : yea, that he has emerged from darkness and chaos of mind into light, from a state of separation from God into union and fellowship with Him ; that, being once darkness, he now feels himself to be light in the Lord. How comes all this about ?—whence this change—this, what shall we say—this transmutation ? His *will*, reason, affections, imagination, are apparently altered. How is this ? Now he has found peace and satisfaction ; the mystery is being solved ; he has found the secret.

The *first* fallen Adam has found the second or last Adam—the earthly has come in contact with the heavenly ; the *living* fallen soul has been quickened and raised unto eternal life by Him who is the quickening spirit—the Son of Man—the Lord from heaven. The power or spirit which brooded over the face of the deep, when darkness rested upon it, has brooded over the deep and darkness of his being ; and the Word has spoken, mysteriously indeed, light into his being, and brought all the latent powers or possibilities of his nature into beauty and harmony, to make all subservient to the design of Him who formed him in His image and for His glory. The notion that a few *simple* monads were first created somehow or other, and that through their working through innumerable ages, by *natural* selection, we thus are made to witness the various and indefinitely multiplied forms of life ; and to be asked to consider this a proof of infinite wisdom and power—rather than the simple divine philosophic and theosophic statements we read in Genesis—is simply absurd. Why should men of science seemingly ignore the beautiful and plain declarations of Scripture in reference to the creation of the universe and of man, in order to bring in vain theories and speculations to attempt its solution ? The whole creation speaks of the *living* Presence of the Living Great First Cause ; and although there are many things which appear contradictory and wrong, yet, rightly understood, we know these are necessary to work out the grand design and show forth the majesty of God. (Hear, hear.) I see this, I feel this. The Book of God I know is in harmony with the Book of Nature ; and when these volumes are thoroughly understood by the truly enlightened mind, he sees a glorious unity in the diversity and sounds in Nature, which were thought to be discordant with the sounds and expressions in the Scripture, but are now felt to increase the harmony and melodize the whole. All is of *One* : God is God ; and His tender mercies are over all His works, which are great, and sought out by all them who have pleasure therein.

Rev. R. THORNTON, D.D.—We must thank Mr. Warington for bringing forward the very interesting subject before us. An accusation some have brought against this Institute is, that we come to our work with foregone conclusions, and do not care for facts. I think our free discussion on the present subject will clear us in some measure from these imputations. Of course we do, in one way, come to our work with a foregone conclusion, because we believe in the truth and inspiration of Scripture ; and we have an avowed object, which is, to examine scientific statements supposed to be inconsistent with Scriptural truths, in order to show that such inconsistency is not real, and disappears when the scientific statements are put into a correct form. For this purpose we stand in need of facts, and are greedy of them ; but we do not want what is often palmed off on us for facts, the crude generalizations and hasty conclusions of sceptical sciolists. Though I am glad that Mr. Warington has introduced this subject, I shall have to trouble you with some remarks which make against him. Still, as a well-known journal has termed him our “*advocatus diaboli*,” I am sure he will not mind the opposition which that *advocatus* must always be prepared

for. (Laughter.) First, let me express my acquiescence in his feeling, that Scripture must not be imported too readily into scientific discussions, but that the two should be considered, as far as may be, separately. I must, however, qualify his words, by excepting those cases where (as with many subjects we discuss) the question turns upon the real force of a Scriptural statement. Here we may see at once that Scripture has to be imported. It would not be unfair (for example), if we were to argue that of two otherwise probable theories, one contradicting, the other agreeing with Scripture, the members of this Institute would naturally adopt the latter. Again, I must thank Mr. Warington for what he has said about geology. I am glad to find Darwinists ready to give up geological arguments. We have had too much of this science; its votaries are far too proud of it. They seem to imagine that a shell or a bone found in an odd place is quite enough to prove Scripture valueless. Mr. Warington gives the right answer to such fancies, by pointing out that our geological knowledge is yet very imperfect; and that arguments drawn from it cannot be alleged either against Darwinism or against Scripture. Let us give them their proper place; but no more.—I am dissatisfied with the title of Mr. Darwin's book, "*The origin of species, by the process of natural selection and struggle for existence.*" What is this struggle? Is there any? (Hear, hear.) Who are struggling? Granted that under certain circumstances the natural powers of reproduction cause a large number of individuals to come into existence; so large that there is not a sufficient pabulum for them, and that some give way, and are utilized in a different manner from others,—is that a struggle for existence? Far from it. They have it; they do not struggle for it, but under certain circumstances cannot maintain it: surely this ought not to be called "a struggle," as if species were imbued with a sort of Ishmaelism,—the hand of each against every other! Another term to which I take exception is, "the origin" of species. Mr. Darwin endeavours to show that species originated in a certain manner, by arguments which really prove that there are no species at all. (Hear, hear.) Mr. Warington himself, arguing as an able Darwinian, says we must not import into the discussion any definition made by prejudice. But he lays down a definition himself, and says we must not assume certain other things, which would be begging the question: "A species is a race of living beings possessing common characteristic differences from all others, which differences at the present time are constant and inherent." This is not adequate. In logical language, we miss the "differentia" expressing the power of reproducing a fertile progeny. To omit such a portion of a scientific definition is really to beg the question, because its omission implies its non-existence; and so the definition from which it is absent is itself a prejudiced definition. And so we find ourselves at issue not about the origin of species, but whether there are any species at all. This further appears from the expressions used with regard to the primeval progenitors of plants and animals. "There may be four or five," "there may have been only one." But these two cases are widely different. If there were five progenitors, then there are species, or may be: if one only, then they disappear. I cannot help thinking that a point has

been here tacitly assumed which ought to be demonstrated ; namely, that there is no species distinguished from another species by the differentia of consistent reproduction, varying only within a fixed limit. Now this I contend Mr. Darwin and Mr. Warington have not proved, and never will.

Dr. J. H. GLADSTONE.—While sitting at the other end of the room I have been thinking of two functions of this Society—two functions it is supposed to discharge—namely, the slaying of giants and the laying of ghosts. By the first I mean, that from the border-land of knowledge and no knowledge, huge theories and hypotheses stalk forth, which frighten many mortals ; and we are disposed to go and fight them. Then sometimes we hear that from a suspicious quarter there has risen something very “uncanny ;” and our wisdom in such a case is to take a candle and walk up and try to put the candle through the ghost’s body, when we generally find the ghost to be something very innocent after all. If we run away from the ghost, the ghost will haunt us, and we deserve it. Now, there is this huge, gigantic and majestic hypothesis of Darwin, and several attempts have been made to slay this giant. Mr. Mitchell tried it in his first Address ; then we have had various questions asked by Mr. Reddie on former occasions, and again to-night ; and since then it has been defended ; and I think now the battle may go on for some time. Then there are various ghost notions about it : some say it is rather infidel ; and there are other ideas about it ; but let us look these suspicions in the face. As to the question itself, it is not to be expected, in a short speech at this late hour, that I can go into it in half its details ; but I have little doubt that if Mr. Darwin had put forth his work as “The Origin of Varieties,” and had insisted that they arose from natural selection, it would have been accepted as an explanation of the origin of varieties by nearly all naturalists, and I do not suppose the question would have come before us here. (Hear, hear.) It appears to me so evidently true that there is this struggle for existence, that there are these modifications taking place from generation to generation, and so true that any modification which is more adapted to the circumstances in which an animal lives must give it a better chance of propagation, that I think there would be no hesitation in accepting natural selection as a *vera causa*. The difficulty springs up when Darwin extends this, and endeavours to push the theory beyond these limits (hear, hear) ; and then comes the question, whether species exist in nature originally, or are varieties carried to such an extent that they become permanent ? That is the question,—a serious one, and difficult to answer. On the one hand, do we find any of these limits of which Captain Fishbourne has spoken ? I do not think we can fix the limits of the power of artificial or natural selection ; and, on the other hand, we do find something like the actual existence of species in nature ;—that is to say, there are allied creatures which are so far apart that they cannot be brought together to reproduce any intermediate creature, or if they have any progeny it is not fertile. But then, again, as to this question of hybridity, we want to be certain about that ; and I think, as experiments can be easily carried on with reference to plants, it will be a fruitful source of inquiry to find the real phenomena. The whole question turns on this, whether

species have a real existence or not in nature ; and it appears to me that our idea of the credibility or incredibility of the hypothesis must almost depend on that. There are various arguments which have been urged against Darwin's views ; and it is difficult to conceive how an eye could be produced by a sensitive nerve : but I need not repeat them. I have no doubt Mr. Warrington can remove a great many of these difficulties, and I do not think he will have much difficulty in answering much that has been said against him this evening. But allow me to produce one argument which appears to me (I do not know why) not to have had the attention given to it which it deserves. It is this. The Darwinian hypothesis absolutely insists on this, that every modification of an animal or plant must, in order to become permanent, be an improvement, and fit it more for the condition in which it is. Hence, to take Mr. Mannes's poetic illustrations, we can understand, on the Darwinian hypothesis, the elongation of the nose of the elephant ; because every elongation of its nose made it better adapted for getting plants. We can understand perfectly well the stag lengthening its neck, so as to become a giraffe ; because the longer the neck the more suitable for getting branches from trees. But then there are various organs which are of no use whatever till they are of a certain development, and there there appears to me to be a great difficulty. I will instance the wing. Until the wing is sufficiently large or strongly developed to be able to lift the creature from the ground and to carry it through the air, it is of no use whatever ; the half-developed wing would be only an impediment. If we go into the history of birds or winged creatures, we of course find that they are made upon the general plan of the vertebrata. We cannot say confidently whether the bird or the mammal came first ; but we know that before birds were on the earth, there were huge quadrupeds of the Saurian order, and abundance of fishes. Now what gave rise to these birds ? Did they come from fishes or quadrupeds ? In any case, it is difficult to imagine that the conversion of the front fins of the fish, or of the fore-legs of the quadruped into wings, would not be inconvenient for the animal ; and that each step would not be a great difficulty in their way ; therefore, the creatures modified in that manner would soon perish, and the birds never be produced. To take a more specific case, that of the bat. We know that the bat differs in ever so little a degree from the mouse,—in scarcely anything except the length of the fore-limbs, and the membrane by which it flies. Now, it is inconceivable that the bat could be produced from anything but a small mammal like a mouse. And, if we imagine the lengthening of the front legs and the formation of the web between the fingers, I think during that process we should get something neither fit for one thing nor another, which would hobble uncomfortably on its thumbs, as the bat now does when on the ground, but without the power of flying. I should like to know what can be said in reference to this. It appears to me a difficulty ; but one which may be only founded on my ignorance and want of imagination ; and perhaps other persons may be able to show clearly how this might have possibly taken place. As to the difficulties that arise from the theological point of

view, I must give my own experience. I read Mr. Darwin's book with much pleasure. I felt the weight of his arguments, and it never occurred to me that there was anything unchristian about it ; but afterwards there arose a ghost, and hearing of it, I took a candle, and it seemed to me the ghost was made of nothing. I could not find out that the first account of creation and the subsequent revelations tell anything as to the way in which God created different beings. The second account gives a specific and detailed history, as far at least as man is concerned. It is unnecessary for us to extend Darwin's hypothesis to man ; and we may accept some of Mr. Manners's remarks, and suppose that God acted in a different way in bringing man into the world. But suppose, with our inexorable logic, we were to consider man's body to have been produced in a similar way, we should find no difficulty in this respect with the first account of creation. With the second account, we have merely to consider that God, in revealing past history, adopted that poetic and figurative style which he always did adopt in revealing future history, and that the analogue of the second chapter of Genesis is not the book of Chronicles or the Acts of the Apostles, but rather the book of Daniel and the Apocalypse. Allow me to finish my remarks with a parable. I took a little child, who had been bred up in a city among houses, for a walk into the country ; and there stood before us a majestic oak. The child said to me, "Who made that tree ?" I said, "God made the tree ;" and in order to give the child, as I thought, some information as to natural objects, and also to raise his ideas of the wisdom and power of God, I explained how that tree was once a little acorn planted in the ground ; that it shot forth and developed leaves and stalks ; and the stalks rose higher and higher, sending out stems and branches, and in this way the whole tree was developed. During all this process, the materials for building the tree were brought to it ; the water in the earth dissolved salts and brought them to the roots of the tree, and so they were sucked up ; and the winds brought carbonic acid and water, and thus the tree grew. But the child turned away and said, "Oh ! I thought God had built up the whole tree at once ; and you say it is being gradually developed, and made out of some other things. I do not think much of God now"—

MR. REDDIE.—I think the child was so far right. The tree it saw was not created ; it grew.

DR. GLADSTONE.—Well, the child got accustomed to the thought, that it might believe in the development of a tree from the seed without being atheistic ; and then, in another walk, I showed the child that the acorn planted was really the fruit of another tree, that had grown from another acorn, and so on ; and then the child, instead of having (as I thought it would) a higher appreciation of the wisdom of God, thought that I had further reduced the idea of God, because this acorn was made from another tree. But gradually it became accustomed to the idea of generation, and that that was not atheistic ; and then, in another walk, I began to explain that as this tree grew from an acorn, and the acorn came from another oak, and tree preceded tree, the trees were not always exactly alike, but that there were

modifications in them ; and that if we went back many generations there were considerable modifications, till we must call the tree by another name than the oak, and that we might go still further and further back ; and that is pure Darwinism. Then the child said again, in almost the same language as at first, "I thought God created every kind of tree at once, and now I find that there has been a gradual development : I do not think much of God." It appears to me that we are in the condition of this child ; and I think we may believe in development, and believe in generation,—that we may believe indeed in this Darwinian hypothesis—without being considered atheists. We know the one, we are not sure of the other yet. I do not know what the fate of this theory may be ; there is much to be said for and against it ; but I have no doubt whatever, that if you speak of this theory as being sufficient of itself to account for all the varied phenomena of creation,—as capable of explaining the whole process,—Darwinism is incredible. But if we accept this theory of natural selection as only a small part of that process which it has pleased Almighty God to adopt in bringing about creation, I think it is neither incredible nor to be thrown lightly aside, nor to be considered an improper theory.

MR. W. H. INCE.—I should not like the evening to close without dissenting from Darwinism, and letting it be known that I cannot believe that only eight or ten original species were created, and that all other species were produced from varieties. In the plan ordered to be followed by Noah in building his ark, it was to be 300 cubits long and three stories high, with lower, middle, and upper stories ; that is, the ark was to be of an enormous size, and a great deal too large for eight or ten species only, if these were all that were required to reproduce all that now exist, as Darwin requires us to fancy or believe. We have never heard throughout the historical period of anything like the development of the elephant or the giraffe, or of any new species. And before we can believe anything of the kind we ought to be told where we may hear of or see some of these developments. With reference to what Dr. Gladstone represented to the child on first seeing an oak in the country, I would ask, Have we ever found the oak to have changed from the elm, or the sycamore, or hop, or from any other of the original trees or plants supposed to have been the first created on the earth ? No. (Hear, hear.) For this, and for many other reasons, without occupying your time further, I should say the theory is perfectly incredible ; and, at all events, I cannot believe it.

REV. W. R. COSENS.—I have listened attentively to the discussion this evening, and arrived at conclusions, which I need not say I have considered before, and one of them I have always entertained. In the first place, I think that we may accept the Darwinian theory, if we put this title to the book of Mr. Darwin :—"The Theory of Deterioration of Species ;" and if, *mutatis mutandis*, we take his book to show in what way the species of mankind may be reduced from high to low, then I think we should be well agreed ; but when we come to consider the way in which the human species (to use his own term of speech) has deteriorated throughout, and the

cause of that deterioration, then I think we arrive at one great, if not the greatest answer, to the whole theory. I mean this: I pass from that one idea which has been prevalent in my mind, to another,—to the idea Mr. Manners struck the chord of, when he spoke of our ignoring any idea of there being a great Saviour-man come to recover man's lost estate. I consider this theory may be good to apply to vegetables and animals and fish, and all the various species with which this world is stocked. There may be causes in climates, in various temperatures, to bring about changes; but when you come to man, you are applying it to a being to whom no law that *you* can in any way bring to bear ought to be applied. I mean this; that the law of man's fall, the law of man's own self-will, what we Christians call free-will, has deteriorated mankind. Let us take the case of man coming from the ark, I mean Noah and his three sons. We have a distinct proof in my mind of the fact that there was a deterioration from that day forth on account of the sin of one man. They came out of the ark, and we find that the descendants of Ham have ever since gone back because of man's sin. It appears that that is ignored, excepting that Mr. Manners alluded to it once to-night. I think, therefore, that this theory is one which ought not to be applied to mankind or what may happen to man.

The CHAIRMAN.—As the time is so far advanced, I shall not call upon any other gentleman to speak; and I think it is only fair—as Mr. Warrington has been replied to in writing, and the discussion to-night has been so long,—that Mr. Warrington should have the same opportunity of consulting Mr. Reddie's written Reply that he had of reading Mr. Warrington's paper. This is also Mr. Reddie's own wish; and I shall therefore, with your permission, adjourn the discussion to our next meeting.

The Meeting was then adjourned.

ORDINARY MEETING, APRIL 1, 1867.

THE REV. WALTER MITCHELL, VICE-PRESIDENT, IN THE CHAIR.

The minutes of the previous Meeting were read and confirmed. Afterwards, the discussion on Mr. WARINGTON'S Paper on "The Credibility of Darwinism" was resumed, as follows :—

The CHAIRMAN.—The subject of Mr. Warington's paper has been so fully discussed, that the time has now arrived when it is my duty to sum up the matter by stating my own views, leaving to Mr. Warington the right of reply. I may express my own views by saying that nothing urged by Mr. Warington in his valuable paper has led me to believe the hypothesis of Mr. Darwin one whit more credible than I ever conceived it to be. The matured opinion I formed, not only after a careful study of Mr. Darwin's book, but after a full consideration of all the alterations and additions of successive editions, remains unaltered. In the first place, I protest against the principle laid down by Mr. Warington, that a hypothesis is to be held as credible unless it can be proved to be impossible, as contrary to all sound principles and to the inductive philosophy of Bacon. I regard this method of procedure as a retrograde step, bringing us back to that system of feigning and inventing hypotheses which was the source of so much error before the time of Bacon ; the abandonment of which, and the procedure of the search after truth by a sounder method, have caused so great an advance in our knowledge of nature since his day. I can find no better summary of the Baconian method of induction than that given in so few words by Newton in the queries appended to his work on optics :—"The main business of natural philosophy is to argue from phenomena without feigning hypotheses, and to deduce causes from facts until we come to the first cause, which is certainly not mechanical." Now the method Mr. Warington (if I rightly understand him) sets before us, is the direct reverse of this. It is, first, to feign a hypothesis, and then see what facts we can find to agree with it, ignoring those that are contrary to it. And though both Mr. Darwin and Mr. Warington do not shrink from an approach to a first cause, Mr. Darwin's followers have not hesitated to disavow a belief in any first cause which is not mechanical. Bacon, like Newton, tells us, that "analysis consists in making experi-

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ments and observations, and in drawing general conclusions from them by induction, and admitting of no objections against the conclusion but such as are taken from experiments or other certain truths, for hypotheses are not to be regarded in experimental philosophy." If the hypothetical method is to be excluded from experimental philosophy, I believe it must be from every branch of natural philosophy, as one unfit for leading the mind to the discovery of truth. Indeed the want of success of the Greek school, which used so freely the hypothetical method, is a caution to those who would have us to retrograde by following their example. According to Mr. Warington's method of arriving at truth, I am bound to accept a hypothesis provided it be credible although unproven; and this, too, though facts seem to contradict the hypothesis, because some unknown but credibly possible circumstances may make the discordant facts accordant. To take his own example,—the perturbations of the planetary system, produced by a planet now proved to exist—viz., Neptune, should not be taken, even if that planet had not been discovered, as adverse to the reception of the law of gravitation; for it is perfectly credible that a planet incapable of reflecting light, and so not discoverable by a telescope, might exist capable of producing the observed perturbations. Now, I maintain that Newton himself would never have held a law of gravitation requiring such a possible credible hypothesis of a planet incapable of reflecting light to bolster it up. The belief that the moon is made of green cheese, taken generally as a proverb of the extremest credulity, becomes, according to this system, a tenable hypothesis; for though extremely improbable, no one can say that it is absolutely incredible. But waiving all objections to Mr. Warington's principle of admitting a hypothesis confessedly unproven by facts, because it is credible and may hereafter be proved, I must confess that on his own grounds that gentleman has failed to make Darwinism credible to my mind. In the first place, let us see what facts in nature Mr. Darwin's hypothesis is supposed to account for. As far as I understand it, it assumes that if we review the whole animate creation, vegetable and animal, and strive to classify the creatures belonging to either kingdom, we shall find the whole so linked together in one continuous chain, unbroken by any breach of continuity, that it is impossible to say, here one species ends, and here another begins. Mr. Darwin's hypothesis to account for this continuous chain of animate beings is that they all sprung either from one progenitor, or as many progenitors as the naturalist can find unbroken chains of animate beings insensibly passing from one change of structure or organs to another. Mr. Darwin feels that he must assume the existence of such chains of organized beings, though he cannot prove their existence, otherwise he would not have to account for the vast flaws in these chains, by the supposition of a vast number of necessary links being lost or undiscovered in the geological strata of past ages. Assuming the existence of all necessary links not found in the present animate creation, either in the defective records of the past or among undiscovered beings of the present, he would seem to think that he can prove the existence of eight or ten such chains. That in all these chains one being not

only succeeds another by almost insensible changes of structure, but that organs found in a rudimentary state in one being are found in perfection in some being further down the chain. He accounts for this continuous chain by the hypothesis that every member of any one of these eight or ten chains has descended from one common ancestor. That the differences to be found between any given members of the chain are accountable for, by a law that any accidental change of structure taking place in a plant or animal is transmissible to its offspring. That, if favourable to the existence of the creature, it will give it an advantage in the struggle of life, and be perpetuated until improved again by accident. That this hypothesis is sufficient to account for all the varieties of structure and for the formation of every complex organ of animal or vegetable beings we may find in the animate world. Assuming that he has proved the existence of some eight or ten beings from which all others have been derived by natural descent, he says in his first edition, "Therefore I should infer from analogy that probably all the organic beings which have ever lived on this earth have descended from one form, into which life was breathed by the Creator." Here, in passing, I cannot refrain from remarking that this admission of a Creator breathing life into one form at least, has disappeared from the later editions of Mr. Darwin's book—

Mr. WARINGTON.—It is to be found in another page.

The CHAIRMAN.—I have not seen it.

Mr. WARINGTON.—It is in the last edition, on another page.*

The CHAIRMAN.—Now I ask, why are we called upon to receive this hypothesis with so little proof? For Mr. Darwin has never given us the proofs of his hypothesis—for these we are to wait for a future work. Why are we to receive a hypothesis so monstrous, so incredible as I conceive it to be? For what other hypothesis is it to be substituted? For this, that instead of the Creator breathing the breath of life into one primordial being, he did it into many. That the surprising uniformity seen to run through the animate creation, is the uniformity of plan of one Divine Creator. That organs and structures have not been formed by chance changes, propagated by the destruction of weaker creatures, but owe the marvellous wisdom, marking their design for the welfare of the creatures in whom they are found, to the direct power of their Creator. It has ever been held as a principle in natural philosophy, that we are not called upon to abandon any law or hypothesis founded on a large induction of facts, till that law or that hypothesis is found insufficient to account for any new facts that may present themselves. Mr. Warington has referred to an analogy—an analogy of which Mr. Darwin seems exceedingly proud—between the simplicity of his own law of the formation of species by what he calls the "Law of the preservation of races in the struggle of life by means of natural selection," and that of the law of gravitation. Notwithstanding

* Mr. Darwin has removed the admission of a Creator of one form at least from the passage where it originally stood in the first edition, in the middle of the work, to a page near the end.—W. M.

the possibility of a dark planet incapable of reflecting light, it has hitherto been held by natural philosophers, without exception, that one discordant fact from observed phenomena, not mathematically accounted for, would be sufficient to upset the Newtonian law of gravitation, although it seemed before to afford a solution for such complex motions of planets and satellites, and so many phenomena of nature as it is supposed to do. Clairvaux was about to express his opinion that the law of gravitation failed, because he found a discrepancy of a small fraction between the moon's observed place and that calculated according to the theory of Newton. Here I cannot help expressing my opinion that our Honorary Secretary has had hard measure dealt out to him, because he has ventured to express his scepticism as to the law of gravitation. He has not expressed his scepticism without giving sound and good reasons for it. Why am I to be so little indulgent to heresy, if it be heresy, in matters of science, when I am called upon to be so charitable to any amount of heterodoxy in religion? That while the Bible may be called in question by any man, and disregarded as the revelation of God's will; while it may be treated as a collection of fables; while its clearest expressions may be regarded as mere apocalyptic visions;—I do not see why, when all this is allowed with so much cold indifference, a man is to be branded as a philosophical heretic because he cannot accept as sound every demonstration of Newton's *Principia*, or Laplace's *Mechanism of the Heavens*. Mr. Reddie gave good reasons, I say, for his scepticism the other evening, but some gentlemen who were present seemed to think I neglected my duty in not calling him to order for wandering from the subject of discussion. A reference to the law of gravitation was a part of the discussion, and Mr. Reddie, I conceive, was quite within the proper limits of the discussion, in maintaining that the law of gravitation was not so incontrovertibly proved as it had been assumed to be. What is the state of the case? The discovery of Neptune in the very place in the heavens where the observed perturbations of Uranus, pointed out by exact calculation on the Newtonian hypothesis that such a planet should exist, such discovery being no casual one, but following directly from the calculations, has been trumpeted forth by scientific men as one of the greatest triumphs of modern philosophy. Now Mr. Reddie calls in question the accuracy of the statements made in all the more modern text-books of astronomy on this subject. He has a perfect right to do so, if he can produce proper evidence. He asserts that the two calculations made by Adams and Le Verrier of the position of the planet causing the perturbations of Uranus, did not by any means agree; that they did not by any means arrive at the same position of the planet; and that the discrepancies in the calculated elements of that planet were considerable; that the planet Neptune, when found, was not in the place assigned to it by either Adams or Le Verrier; and, finally, that the elements of the orbit of Neptune, as determined from observation, differ so considerably from those calculated by Adams and Le Verrier, that they cannot be made to agree with either. I maintain, therefore, that our Honorary Secretary is perfectly philosophical in urging these facts against this argument for the Newtonian

theory of gravitation. If these facts be true, the much-vaunted discovery of Neptune is no triumph of abstract science. Every fair defender of the Newtonian theory is bound to show, either that the observations of the new planet on which the calculations of the elements of its orbit have been made are faulty, and the deductions made from them false, or else he is bound to demonstrate that both Mr. Adams and M. Le Verrier were mistaken in their calculations of the orbit as deduced from the perturbations of the planetary system. In neither case, if the facts and figures quoted by Mr. Reddie be true, can we maintain for an instant that the calculations of Adams and Le Verrier confirm the Newtonian law of gravitation, or were any real triumph of the powers of modern analysis. In confirmation of my view of the unphilosophical procedure of Darwinism as departing from the principle of the inductive method of arriving at truth, I would venture to allude to another science with which I have no doubt Mr. Warington is as familiar as he is with that of astronomy. It is a rule, as I believe, always followed by all sound cultivators of natural philosophy, that a hypothesis is only considered tenable so long as it accounts for all observed facts. Why has the undulatory theory of light been allowed to replace the emission theory of Newton? The emission theory of Newton accounts for a far greater assemblage of observed facts and phenomena relating to light, I venture to say, than that of Mr. Darwin on the origin of species does for the phenomena of animal and vegetable life and structure. There are a vast assemblage of phenomena—not one or two merely, but a vast number—which can be strictly and mathematically demonstrated to be direct consequences of the emission theory of light; that is, the theory, that light consists of material luminous particles emitted from a luminous body. But why has this hypothesis—so simple, clear, and beautiful in itself, and recommended by such a master mind as that of Newton—been so universally abandoned by modern physicists? Because experiments demonstrated the existence of a series of facts for which the emission theory could give no explanation. On Mr. Warington's hypothesis, the Newtonian theory of light is not only credible, but the contradictory facts might be made to accord with it, by some unknown undiscovered causes. Indeed, Sir J. Herschel—the most prominent supporter of the undulatory theory—has remarked, that the defect might lie not in the facts being discordant with the emission theory, but our want of power in pursuing the mathematical analysis of Newton so as to explain them; regarding that analysis as like the bow of Ulysses, which none but its owner could wield. Instead of doing this, however, modern philosophers have abandoned the theory of Newton and adopted another, which not only includes all the facts shown to be in accordance with the emission theory, but also the great majority of those facts observed since the time of Newton, for which his theory afforded no explanation. But even here we pause. The undulatory theory itself is not yet universally accepted, as a few facts are still left for which that hypothesis does not afford an intelligible explanation. I assume, therefore, that I am right in maintaining that that system of inductive philosophy, which has led to the boasted

discoveries of modern science, requires the rejection of any hypothesis which does not account for the whole range of known facts to which it relates. Now, I ask, is there no other hypothesis than that of the Darwinian which will account for all the observed phenomena of animal and vegetable life and structure? I have shown that there is one: one, too, possessing this advantage, that while it includes all the facts urged in favour of the Darwinian theory, embraces all those which cannot be deduced from that theory, and many which are directly in opposition to it. I say that there is such a hypothesis. I have already brought it before you. Why are we to abandon it? Upon what sound scientific or philosophical grounds? Because it is too narrow to account for newly-observed facts or phenomena? No. But because it is considered to have too theological an aspect! Therefore, we must take another, less theological, deduced from a narrower range of facts, and leaving out others strictly included in the rejected one. But why should we be afraid of the theological aspect of a theory? Why adopt another which drives the operations of the Deity a little farther from our ken? Are there not great philosophical truths which man cannot possibly ignore, though they do lead him up to the more immediate contemplation of the work of his Creator? If the visible things of the world have plainly imprinted on them the fact that they are creatures of an invisible Creator—a Creator almighty in power and infinite in wisdom—I say if that fact be written clearly and intelligibly on all that we call the works of nature, surely we have no right to exclude that grand, general, most patent fact, because in the present day it may be regarded by some as too theological, or as introducing a theological bias into science. If science be another name for real knowledge—if science be the pursuit of sound wisdom—if science be the pursuit of truth itself—I say that man has no right to reject anything that is true, because it savours of God. Well, what is this hypothesis—older than that of Darwin—which does, and does alone, account for all the observed facts, or all which we can read, recorded in the book of nature? It is, that God created all things very good—that he made every vegetable after its own kind—that he made every animal after its own kind—that he allowed certain laws of variation, but that he has ordained strict though invisible and invincible barriers which prevent that variation from running riot—and which include it within strict and well-defined limits. This is a hypothesis which will account for all that we have learnt from the works of nature. It admits an intelligent Being as the author of all the works of creation, animate as well as inanimate: it leaves no mysteries in the animate world unaccounted for. There is one thing which the animate as well as the inanimate world declares to man, one thing everywhere plainly recorded, if we will only read it, and that is the impress of design—the design of Infinite Wisdom. Any theory which comes in with an attempt to ignore design as manifested in God's creation, is a theory I say which attempts to dethrone God. This the theory of Darwin does endeavour to do. If asked how our old theory accounts for such uniformity of design in the midst of such perplexing variety as we find in nature, we reply, that this can only be

accounted for on one admission—that the whole is the work of one Author, built according, as it were, to one style: that it represents the unity of one mind with the infinite power of adapting all its works in the most perfect manner for the uses for which they were created. It would not be difficult to show that this hypothesis guided Cuvier in those investigations of comparative anatomy which led him, from one or two bones, to build up accurately a skeleton which he had never seen. It was a doctrine held by Hunter, the father of modern physiology, as it had been by his great predecessors in that branch of science. How did they attempt to discover the use of an organ? Was it by tracing it through an infinite variety of chance changes? No. They sought for and arrived at that knowledge by assuming this as an axiom—as a canon for the interpretation of the structure of the animate world—that every organ in every animate structure was adapted by Infinite Wisdom for its own particular use. It was by believing this—it was by a firm faith in this wisdom, in this adaptability of organs, in this perfection of their design—that all the greatest discoveries in physiology have been made. Whewell has boldly maintained, and he has never been controverted, that all real advances in the science of physiology and comparative anatomy—such as that made by Harvey in discovering the circulation of the blood—have been made by those who not only believed in the existence of design everywhere manifested in the animate world, but were led by that belief to make their discoveries. On the other hand, what great discoveries, may we ask, have been made by those who deny design and believe only in the self-evolving powers of nature? Is not *our* hypothesis, then, a good working hypothesis? Are we called upon to reject it for another which has neither worked so as to produce an advance in our knowledge nor yet can be twisted to account for facts diametrically opposed to it? But why should we put it so low as a hypothesis? I believe it to be something much higher than a hypothesis. I believe it to be clearly and plainly revealed as a truth given by God to man in His own book. I believe with a great master in science, that the man who cannot perceive that such an organ as the human eye manifests such a perfection of design that it could only come from the mind of an infinite Creator, is a man possessed of an ill-regulated, ill-constituted mind; that his mental vision is subject to a far greater defect than could be compared with that defect of vision called colour blindness. Let us further test the credibility of Darwinism on issues raised by Darwin himself—such, for instance, as the formation of the human eye on his hypothesis. “If it could be demonstrated,” he says, “that any complex organ existed which could not have been formed by numerous successive slight modifications, my theory would absolutely break down.” The whole tenor and spirit of all that Darwin writes on this subject may be thus paraphrased:—The argument from design is the greatest crux I have to get over; I must evade it or deny it altogether—design can have no place in my system: admit it, and my hypothesis falls to the ground. He admits that if such a complex organ as the human eye could not be formed, as he says it has been, by the law of natural selection, his theory must absolutely break down. Besides the

formation of the eye, he tries his system by another crucial instance—the instinct which leads the hive-bee to construct its marvellous geometrical cell. By these two instances, challenged by Mr. Darwin himself, would I test his hypothesis, and try not the truth, but the credibility, in Mr. Warington's sense, of the system. How upon this system is so complex an organ as the eye formed? The primordial being of Darwin is not formed with any eye from which our own may trace its ancestry. It is to be traced back to an organ not optical at all, or made with any reference to the laws of light—but to the mere chance exposure of a nerve of sensation to the influence of light. In making our first step backwards from the human eye, Mr. Darwin tells us it is not a perfect optical instrument—that it is imperfect in achromatism. Now, though I am aware that Sir D. Brewster is stated to have admitted that the eye is imperfect as an achromatic instrument, I would venture to question that assertion. I know that my own eyes are not now perfectly achromatic; but they once were so—as I know by my own experience. But they have acquired this defect, as they have that of short-sightedness, by an abuse of their proper use. I assert that the human eye is a most complex organ, regarded as an optical instrument—that it combines in one all that man strives humbly and vainly to imitate, in a perfectly achromatic microscope, telescope, and camera obscura. I maintain that all the conditions which pure mathematical science can demonstrate as necessary for destroying spherical or chromatic aberration in the differing densities, curvature, and distances of lenses, are perfectly fulfilled in the human eye; that, too, in a manner defying the imitation of human art. And why? Because man cannot make a lens out of a substance varying in refracting powers. He cannot even grind his lenses to the proper curvature which his mathematical analysis teaches him to be necessary. But such defects are not to be found in the workmanship of the eye. I therefore take the eye, as I believe I have a right to do, on sound scientific principles, as a perfect optical instrument. I say nothing of the secretion of that black pigment which absorbs the superfluous rays of light. I say nothing of that marvellous mechanism which changes the curvature of the lenses of the eye in a manner no human instrument can ever do. I say nothing of the iris—that varying diaphragm so sensitive to light, not for vision but for contractibility—which admits into the camera obscura of the eye just that amount of light which is necessary for the perfection of the image on the retina. I take this marvellous instrument, and I am told by Mr. Darwin that his system must collapse, that his hypothesis must crumble to dust, unless I can believe, as a thing within the range of credibility, that this perfect instrument has originated without a designer. For this is the force of Mr. Darwin's argument—that these lenses, so perfectly adapted to the laws of light, in geometrical form and refractive powers on the rays of light, with all the marvellous mechanism for adapting them for near and distant vision, manifest no unanswerable evidence of design—that it is credible that all this marvellous combination and perfect adaptation to the laws of light are due to no forethought—no design—no wisdom. That all this has been formed simply by the law of

natural selection. That some being possessed of sensitive nerves some æons of ages ago, had one of these nerves accidentally exposed to light. I am told, without proof, that any nerve of sensation—by which I presume is meant a nerve sensitive to the touch—if exposed to light would be sensitive to light. That this nerve becoming so sensitive to light, became protected by a transparent film. That I must admit these assumptions, contrary to all we know about nerves of sensation, as credible. That starting from such an imperfect eye as this, I am to arrive at the human eye according to this law; that an animal possessed of such an imperfect eye as a nerve covered with a transparent film would have such an advantage in the fierce struggle for existence as to destroy all its eyeless congeners—that it would necessarily propagate animals with like imperfect eyes—that in the course of time, if any accidental improvement took place in the film better adapted for the purposes of an eye, the animal with the improved eye would succeed better in the struggle for life, and propagate successors with the improvement. And so the chance improvements occurring through no law of design, but seized upon by the stern law of the fierce battle for existence, during a succession of uncountable ages, is sufficient to render the formation of such an instrument as the human eye credible. I ask for proofs of so monstrous an hypothesis—something to render it credible. I am told that animals exist having eyes far more imperfect than those of man. But the series which is to set forth the slow steps of successive improvements of the eye are not to be traced in the present great variety of eyes now found among the animal creation. There are breaks in the law of progression. In one direction I may start with one eye, then eight eyes, then countless myriads of eyes or lenses, in the same living being. How is it, in the formation of the eye according to this principle of chance improvements, when I trace the eyes of so great a proportion of what are called the higher animals I find this law of divergence strictly confined to the number two, while among the lower orders of the animate world it ranges through such a wide variety? Why such uniformity in one direction? why so great a variety in the other? Again, setting aside this difficulty, and supposing that the missing links of a series of imperceptible gradations are buried in the undiscovered strata of past geological ages, I ask, why do the animals with the eyes taken as examples of imperfect ones, still survive in that battle for existence in which they ought long ago to have been worsted? But here I would pause, and ask whether the eyes taken by Mr. Darwin as imperfect eyes are so? I deny their imperfection—I believe they are as perfectly adapted to the wants of their owners as my eyes are to mine. I believe the eight lenses of the spider, or the millions of lenses of the bee or the butterfly, are as perfectly adapted to the necessities of those animals as man's or those of any other being. I know that if I search for the microscopic lens invented by Coddington from his knowledge of the laws of optics, in the works of animate nature, I find it in any one of the lenses of the eye of the common house fly. But if it be credible that such a complex organ as the eye is formed in this way, I must assume all other complex organs to be created in a similar manner. The ear is thus formed

without any respect to the laws of acoustics—the heart without any reference to the laws of hydro-dynamics, though the ear and the heart display instruments as perfect—one with regard to the laws of sound, and the other to the most recondite problems of hydro-dynamics—as the eye manifests in regard to optics. But where is this backward imaginary pursuit to lead us—to an animal without eyes, without ears, without heart? Can we stop here? Must not nerves and arteries themselves have started from some chance production of such things in an animal destitute of them? Must we not go back to a monad with a homogeneous unstructureless mass of matter, in which life alone has been flashed by the Creator, and left to perfect itself, controlled only by one stern law, and profiting by no law of design, but simply that of chance? Darwin would fain lead me back to this one simple monad as the progenitor of all the creatures of the animate world. But he admits that his proof, the proof credible to his own mind, fails him. He admits that his researches cannot reach this simplicity. He must start from some ten or twelve such commencements of life. But if from ten or twelve, why not twenty—if twenty, why not a hundred? Why am I to limit the work of the Creator to the simultaneous or successive creations of ten or twelve commencements of the animate creation? Why, simply for the purpose of evading the evidence of design as manifested in the adaptation of all the organs of every animate creature to its wants, which can only be done by so incredible a hypothesis as that of Mr. Darwin. I say fearlessly that any hypothesis which requires us to admit that the formation of such complex organs as the eye, the ear, the heart, the brain, with all their marvellous structures and mechanical adaptations to the wants of the creatures possessing them, so perfectly in harmony, too, with the laws of inorganic matter, affords no evidence of design—that such structures could be built up by gradual chance improvements, perpetuated by the law of transmission, and perfected by the destruction of creatures less favourably endowed—is so incredible that I marvel to find any thinking man capable of adopting it for a single moment. Mr. Darwin not only deprives us of any evidence of design in the physical structures of animate life, he would also eliminate that evidence from the psychological phenomena of living beings. He feels bound to bring the cell-making instinct of the hive-bee within the working of his hypothesis. He does not deny, as some of his admirers have endeavoured to do, the mathematical perfection of the cells constituting the honeycomb. He does not seek to evade the problem by the fiction of equal pressures, exerted by equal hemispheres pressing against each other. He does not ignore the fact that the angles of the terminal planes of the hexagonal cells were determined and measured long before there was any hypothesis as to their formation, and even before the mathematical problem was solved which showed that the bee's cell was the only form which gave the greatest amount of store-room with the least possible expenditure of material. How does Mr. Darwin account for the hive-bee acquiring this marvellous instinct for making so perfect a mathematical structure? Why a chance improvement in cell-making manifesting itself among a certain set of

bees, gave them an advantage in the struggle of life above other bees? This improvement was transmitted to the next generation. Then another improvement was made in the same manner; and so on, till, in process of time, as an accidentally exposed nerve became a perfect eye, a race of bees gradually improved an almost shapeless cell into the mathematical perfection of that of the hive-bee. But the incredibility of this hypothesis, tried by so crucial an instance, seems even to strike Mr. Darwin. He strives, therefore, to render it more credible, by detracting in some degree from the perfection of the hive-bee's instinctive work, and to make the bee work its structure by what he conceives simpler principles than have been observed in the actual operations of that insect. But the perfection of the bee's cell does not consist simply in the economy of material produced by uniting hexagonal cells together. The wasp, building its comb with paper cells, and having the material for the manufacture of this paper always in abundance, is content with this degree of economy. It builds each comb parallel to the other, but it does not in that comb introduce a double row of cells—each hexagonal cell being terminated by a plane or flat surface. The hive-bee, on the other hand, making its wax out of a material requiring great industry in its collection, to be found only for a short time while flowers are in full blossom, having to manufacture the raw honey so gathered, and secrete it like milk, after a digestion of it in its stomach, has to use more economy in the structure of its comb with wax than the wasp using paper. It makes each comb of two sets of cells placed back to back. Each cell is terminated by three flat lozenge-shaped planes, each plane being shaped like the diamond on playing-cards. The three planes terminating a cell on one side of the comb, are the bottoms of three different cells on the other side; so that the hexagonal cells are not placed back to back. Indeed, the partition wall of the two sets of cells forms a series of lozenge-shaped cups on either side, and gives marvellous strength to the structure of the comb, on the same principle which causes the Gothic architect to support the weight of his roof by flying buttresses. A thousand, nay, a myriad of angles might be chosen for the rhomb-lozenge, any one of which would imitate the structure of the bee's cell as to its general appearance. Rigid mathematical evidence shows, however, that the bee chooses just that one angle of $109^{\circ} 28'$ which gives the greatest economy of material with the greatest power of storage. Indeed, the mathematicians made a mistake in their problem, and took the angle $109^{\circ} 26'$ as the perfect angle. Then it was said that the bee was nearly right. But after all, the mathematicians were wrong! A miscalculation in a table of logarithms was the cause of their blunder, and the bee was demonstrated to have chosen the proper angle, accurate to a minute of a degree. Now Mr. Darwin strives to simplify the bee's problem by one of his own, which he takes to Professor Miller at Cambridge, as to the intersection of equal spheres. He gets that professor's solution as to the distance of the intersecting radii; and then, assuming that the bee has contrived to calculate the square root of two to five places of decimals, he supposes that the whole instinct producing this marvellous structure can be brought credibly

within the range of his hypothesis. I would, however, submit that the bee finding the square root of two to four or five places of decimals is as marvellous as anything Mr. Darwin seeks to explain away. But though I do not suppose that the bee is acquainted with our decimal system of notation or can count the measures of its cell, I will concede that the structure of the lozenge of the bee's cell gives not approximately, but most accurately, the square root of two and the square root of three, not numerically, but geometrically. If you take the side of one of the lozenges terminating the bee's cell for a unit, you will find that half the smaller diagonal of the lozenge is accurately the square root of two, and half the larger diagonal is the square root of three. But conceding all this, supposing the bee knows the exact radius to choose in excavating its hemispheres on both sides of a wall of wax, Mr. Darwin sets the bee to the solution of a most difficult problem in geometry; one, the difficulty of which he will only appreciate if he endeavours to solve it with the aid of good mathematical instruments. The problem is this:—Given a wall; to find a point on one side of the wall, supposing the wall of the same uniform thickness and bounded by parallel planes equidistant from three equidistant points on the other side. Let any of my auditory attempt the solution of this problem, even with line and compass, and they will learn to marvel only the more, if the bee does take this method of constructing its cell. But where are the bee's compasses, where its accurate rulers? I remember hearing the architect of the British Museum discoursing on the marvellous structure of the bee's cell before the most distinguished architects in London. Some one talked about the bee excavating a hemispherical cell as the first step of the process. The learned lecturer asked where were the instruments which would enable the bee to make such a structure. Nay, he asked how many of his auditory could make an accurate circle without a pair of compasses; much less excavate a hemispherical cell out of a mass of wax. I bring this to your consideration to show the perplexities and mistakes learned naturalists, such as Darwin, undoubtedly can fall into. Because here mathematical considerations enable me to demonstrate the incredibility of such explanations of the bee's marvellous instinct. Mr. Warrington has told us a good deal about the transmission of acquired habits by animals. I do not think, however, that he demonstrated such habits to be transmissible. But let us assume that they are. Let us assume, too, that some mathematical bee solved the problem of the perfect cell. How was this acquired habit transmitted? Not by that bee, assuredly. The hive-making bees are females indeed, but they are imperfect, sterile females, incapable of propagating their species. Transmission of acquired habits, therefore, could not have anything to do with the perfection of the bee's cell. Tried here by this fact alone, the crucial instance selected by Mr. Darwin fails. It becomes utterly incredible, even by the laws of his own hypothesis. Again, why does the bee, showing such economy in the construction of ordinary cells, ignore this economy altogether when forming the cell of the future queen?—a bee that will never excavate a cell, and yet be the parent of a whole hive of architects? Again, we may ask how does Mr. Darwin's hypo-

thesis account for the fact that in this country alone more than 250 species of wild bees have never built combs of such mathematical perfection? Why have the makers of simpler cells not succumbed to the hive-bee in the dreadful struggle for existence, which Mr. Darwin would elevate to the place of that wisdom which has impressed the hive-bee with its marvellous instinct, as it has placed stars and planets under the laws which regulate their orbits?—But why have these 250 species not been consigned to the records of past geological ages? Mr. Darwin brings his system to the test of two crucial instances—the first the formation of complex structures; the second the production of instincts evidencing the same marks of design, as such organs as the eye and ear. He does not confine himself to the eye among animal structures. The fish is the parent of the mammal, the lung but the improvement of a swimming-bladder. Thus he would eliminate all evidence of design, not only from the structure, but also from the guiding instincts of the animal and vegetable world. Tested by these two instances, I feel that my incredulity of Mr. Darwin's hypothesis becomes strengthened into a conviction, not only of its improbability, but its absolute impossibility. The hypothesis that the Creator made the whole animate world spring from many centres of being; that he endowed each parent form with the power of propagating its like; that he furnished each of these with all fitting instincts for their preservation; that he endowed the living structures not only with the principle of life, but with the power of repairing and reproducing these structures in all their perfection of wisdom and design,—this surely offends no philosophical or scientific canon. Surely it cannot be more philosophical to limit the power of the Creator in giving life, to confine that power to one solitary instance, or at most to ten or twelve. My hypothesis concedes to the Divine Creator the power of creating as many forms as He saw fit in His Divine wisdom to create. It accounts for the marvellous uniformity of type and design in the midst of as marvellous a degree of variation and adaptability, by the supposition that this is an evidence of one Maker, one origin. There is another consideration which, to my mind, manifests most clearly the incredibility of Darwinism as a scientific hypothesis. We are told by Mr. Darwin, and Mr. Warington has repeated the same thing, that man can exercise a law of natural selection when improving the breeds of domestic animals; that improvements so produced by man's interference are transmissible from one generation to another. And we are told, if man can exercise this power to a certain extent, that Nature can do the same if we allow but time enough for her operations; the term Nature being used, not as synonymous with the directing power of the Divine mind, but being simply the law of natural selection exercised by the fact of the weaker and less-endowed animals being destroyed in the battle of life or struggle for existence. Mr. Warington tells me that I am not at liberty to assume that there is any limitation to this law of variation and transmission of changes of structure;—that if I do so, I am begging the whole question which is in dispute. Now, while I concede that man can produce certain variations, I find it is a stern law of Nature that he can only do so within

certain very narrow limits. That Nature presents everywhere certain barriers which all man's power of reasoning and all his skill in selection will not enable him to pass. That it is a fact, if we take animals of two different species, that these species will produce a progeny, if the two species who are the progenitors are not remotely separated from each other in their general structure. The mules, however, thus produced are sterile and incapable of propagating their species. Now this indisputable fact Mr. Warington tells me I am not allowed to make use of in refuting Mr. Darwin's hypothesis, because that hypothesis, if true, calls that fact in question. But I maintain, in all strictness of philosophical investigation, this fact, if it be a fact, cannot be ignored. That the supporters of Darwinism are bound, as the very first step to the admission of their theory as a credible account of the origin of species, to show that no such limitation as this exists in nature. Mr. Darwin refers to the pigeon. He shows the wondrous changes that man can produce in the breeding of that bird, all which we readily admit ; no one has denied it, and its admission is nothing new. Man, watching with great care certain peculiarities in two pigeons, and pairing these, making a similar selection among their descendants, and so on, produces a great variety of pigeons, each variety, under careful pairing, capable of reproducing its own peculiarities. This is true also of the dog, the horse, the sheep, the fowl, all animals domesticated by man. But, admitting all this, what does it amount to ? Has man, with all his skilful application of this law of natural selection, been able to change the species of any one of these animals ? His pigeons still remain pigeons, his dogs remain dogs, and horses remain horses. He has been unable to make any change of structure capable of being construed into a new species. Has he been able to make any such change as that, for instance, which causes the swimming-bladder of a fish to be converted into the lung of a mammal ? But Darwin asserts that man could do all this if we gave him a certain greater length of time than the historical period of man's existence would give us. He also seems to imply that man could step over this barrier of nature, if the new species of animal were pre-eminently useful to man. There is an instance, however, which directly negatives this latter assumption. The horse and the ass are two different species of animals, according to the usual and natural definition of difference of species ; that is, two animals being considered as different species if their hybrids are infertile. Now between the horse and the ass there is apparently no greater difference of anatomical structure than exists between say the Shetland pony and the big dray-horse. According to Mr. Darwin's own views, the horse and ass come from a single ancestor not very remotely removed from either. Now it must be admitted on all hands that the mule in many countries is a more useful animal to man than either the horse or the ass. Yet man, with all his skill, all his watchfulness of the law of variation, and power of adapting it, has never produced a fertile mule capable of propagating say a new species of animal like the mule. Man, with all his power of interference, has never been able to get over this stern barrier of nature—this limitation within strictly defined boundaries, not of accidental varieties, but those urged by

man's power of intellect and arbitrary interference. This barrier is admitted by Darwin himself, and I say it is fatal to the credibility of his hypothesis. What are his own words? "It is difficult," he says, "perhaps impossible, to bring forward one case of the hybrid offspring of two animals being perfectly fertile. I doubt whether any case can be authenticated." I ask, is not this an admission that there is a natural limitation to the variation of species? If this exist where you have the power and the skill of man's interference, does it not exist where you have no such powerful interference? For man assuredly can do more in a few years than Darwin's blind law of natural selection, unguided by any intelligent power, could do in scores of ages. I say, therefore, that this admission is fatal to the credibility of Darwin's hypothesis; that it shows that there is a natural distinction and an impassable one between natural species of animals. Some have thought to break through this law by adducing supposed cases of fertile hybrids from the vegetable kingdom. But these supposed instances have been shown to be ambiguous, and by no means determined with any degree of rigid accuracy. But there is another law admitted to be a matter of observation, even by Darwin himself—that law is, that the accidental varieties produced in the same species of animals and plants by man's interference have always a tendency to revert back to their original type, the moment man's arbitrary interference is withdrawn. We might well indeed question whether man's artificial varieties are improvements of the plant or animal so far as its nature is concerned. His prize-ox or pig may be a better manufacture of flesh and muscle, but I doubt whether these changes are better or more conducive to the health and well-being of the animals themselves. This may be a reason why Nature resents these arbitrary interferences, and tends to bring back the creature to the more healthy type. The infertility of hybrids, and the tendency of artificial variations produced by man in the animate world, both vegetable and animal, to revert to their original type, are two great facts which prove that Darwinism is an incredible and untenable hypothesis. Analogous contradictory facts, if they could be adduced against the theory of gravitation, would be sufficient to render that theory untenable. Analogous contradictory facts have caused the undulatory theory of light to replace Newton's emission theory.—In the observations I have made, I think I have shown that Mr. Warington's four tests of the credibility of Darwinism all fail. I believe that it fails in *possibility*, for I cannot admit that such organs as the eye, the ear, the heart, could be formed without an intelligent designer, or by the law of natural selection. I cannot admit the possibility of the formation of the spinning spider's spinneret, with its power of producing thousands of parallel strands far better than any cable invented by man, could arise from any such law. I cannot admit the possibility of such instincts as those of the bee, and other instincts of animals evidencing quite as wonderful an amount of intelligent design being produced or propagated by any such law. Not only is it impossible to conceive the law of natural variation producing such effects, but further, it is inadequate to account for some of the most patent phenomena of animal life. I have shown also that it is incon-

sistent with the great phenomena of nature. The existence of thousands and thousands of what this theory is pleased to denominate imperfect animals side by side, undestroyed in the great hypothetical struggle for existence, is an inconsistency which has not been touched upon, and for which not the slightest explanation has been accorded. The lowest forms of life—those containing the simplest forms of organs that the records of the rocks have shown to have existed from the remotest periods of the earth's history—are those found still in existence, as simple, as unimproved by the law of natural selection, as ever they were. The very creatures that ought from their weakness to have been "improved off the face of the earth" by Mr. Darwin's stern law, still hold their own; while the more gigantic and powerful—the huge saurian, the colossal mammoth, the armour-plated armadillo of vast dimensions—these and their like have perished, while their weaker representatives remain as an evidence that some other and better law than that of Darwin's must be sought for to explain the phenomena of the animate world. I can find no *harmony* whatever in this theory; to me it is full of jarring discords. Study the animate structures of the world as they are displayed to man's intelligence, either in those of the present existing plants or animals, or as manifested in the records of the rocks showing their history in the past, and you will find a hymn of the profoundest harmony running through the whole. The men who have best interpreted the laws of animate nature have been those who had the keenest sense of this harmony,—the perception of the harmony arising from the perfect wisdom, the perfect evidence of design, running through the whole of the Creator's handiwork. Newton and Kepler could catch the sound of the music of the spheres; Galen admitted the hymn of nature. The greatest discoveries in science have been made by men whose souls were filled with the perception of the universal harmony pervading creation. For what is harmony? Does it not involve the perfection of symmetry? What is musical harmony? You say two sounds are harmonious when they make a perfect concord in your ears. Two sounds may produce what we call a discord, two others a concord; the one most unpleasant and the other most agreeable to our sense of sound. What constitutes the difference between them? The physical philosopher tells us that the concord is produced by two vibrations having a certain mathematical coincidence in certain simple proportions between their waves. Moreover, he can make the musical harmonious note register itself on a vibrating plate or string, and manifest itself to the eye by various contrivances. But all these show a geometrical form of perfect symmetry, causing us to recognize the fact, that the perception of harmony by the ear is caused by its power of interpreting the symmetry of the vibrations producing the harmonious sound. Hence the effect produced on the eye by the painter's skilful arrangement of colour, and of light and shade is called *harmony*. But I hold the existence of higher harmonies than those of ear and eye. I believe man to be created with the power, if he will cultivate and use it, of perceiving the subtle harmony, subtle and inexpressibly beautiful, which runs through the whole of creation. What is this beautiful subtle

harmony? It is the perception of the manifestations of infinite wisdom evidenced by that design upon which all those works are framed. The man who can rightly trace the design manifested in the adaptation of his own eyes to the laws of light,—the anatomist who can read a similar perfection of design in every animate structure,—is the one who can feel ravished by the sublimest harmonies which the soul of man can perceive. He finds symmetry and harmony ruling everywhere. To him, like the Psalmist of old, these harmonies tune his affections to perceive the still higher harmonies of God's love and care for the spiritual as well as the material well-being of his creature, Man. I say, Darwinism is to me incredible, because it would rob me of all perception of this harmony—a harmony the sense of which I believe to have led the best observers of nature to a more intimate knowledge of the animate world—a harmony which has led to a greater perception of truth than any other human endowment. The men who have been the best interpreters of nature have ever been those, I believe, who have the acutest perception of that universal hymn, sung by all God's works, "The hand that made us is divine!" (Applause.)

Professor MACDONALD.—Sir, I thoroughly agree with you in the great object that you have had, of illustrating the great truth of God in nature. But I am going to put a small objection to the dogmatism with which you have laid it down, as against the reception of this hypothesis, from the examples already given of the calculations of two great discoverers of modern times, in regard to the planet Neptune. You pointed out that Adams and Le Verrier had acted each upon their own independent views and calculations, and had hit upon the spot where this planet was to appear—

The CHAIRMAN.—Pardon me, Professor Macdonald; you were not present on the last occasion, and therefore did not hear what I was referring to. It was an argument of Mr. Reddie's; and it was this (the very reverse of what you are stating), that the calculations of Le Verrier and Adams disagreed with one another; and that when the planet was discovered, the discrepancy was found to be still greater between its true place, &c., and that given by either of their calculations.

Professor MACDONALD.—I merely say that a theory should not be abandoned until proved to be useless. I assume that so long as a theory may assist us, from its credibility—(hear, hear)—we may make use of it; but it must fall when it fails in producing truth—

The CHAIRMAN.—That is just what I was maintaining.

Professor MACDONALD.—I wish to have a saving clause against the condemnation of an apparently incredible theory. With regard to the question, which I have only picked up from what I have known of Darwin's theory, I quite agree with you in not receiving the theory. I think that Darwin, one of our best observers, one who has traversed and navigated the globe, and given the greatest possible sound information upon natural history, has unfortunately stretched a small pigeon-theory beyond what he ought to have done. You have also pointed out the hybridism of the different races, and how that has been effected. Who was it, as you have said, effected that?

God's representative in nature, God's vicegerent upon earth—man ; and man has been able so far to go, but no farther. Another view that I quite approve of in your general observations, is with regard to the objection to the theological interpretation. Now, when there is a different feeling, which seems to be getting abroad, in refusing to make use of the interpretation of the Bible for the benefit of natural philosophy, I think it is a pity that the theological departments should continue to a great extent to repudiate the most important chapter in the whole record. The first verse in the first chapter of Genesis I hold to include the most important fact that has ever been recorded. It shows the perfect existence of God, and His necessary creation of the universe. "In beginning"—the very initiation of beginning, the very first instance at which creation was to be exerted—"In beginning, God began the universe." That is slightly phraseologically different from the words of our translation, but I think it is in perfect accordance with the original Hebrew. Now I think it is a pity that there are those who would wish to separate the proper reverential application of the Scriptures, even from experimental philosophy and the common business of life ; for it is our charter. (Hear, hear.) The Church and theologians took it up for a long time, and kept it as their Divine right to hold ; but it is now thrown on the world. It is our charter ; we can use it for everything : it contains truth ; and God is the source of truth ; and therefore I always regret that, if not ignored, they have at least silently admitted the influence of those who dislike the Bible, and who say the first chapter is a mere myth. We all begin from the second chapter, and they say the first and second chapters are the same, as if some historian should say that when the first Napoleon returned to Italy after his brilliant successes, and when the present Napoleon also returned from Italy after his great success, Napoleon I. and Napoleon III. were the same person. So they say that the man recorded in the first chapter of Genesis, the man created on the sixth day, was the same as Adam, the individual man whose creation is narrated in the second chapter—that those were the same people. It would be a parallel case, and I regret that ecclesiastics do not read the whole thing as it is—read it as they do any other historical book containing true facts. This is the first opportunity I have had of appearing at this Institution. I was pleased when you were good enough to bring it under my notice, for I quite cordially agree with its objects, which seem to have been well carried out as yet, and I wish it all success.

The CHAIRMAN.—I shall now call upon Mr. Warrington to reply.

MR. WARRINGTON.—I must ask you to bear patiently with me, because I have the discussion of two whole meetings to answer, and if I do not reply as fully as I intended, it is owing to the lateness of the hour, for I imagined I should have had at least half this evening at my disposal. I begin with Mr. Reddie's paper. Mr. Reddie wishes he could have changed places with me. That cannot be ; but I can undertake to give him one-half of the pleasure he would thus have derived ; for although he cannot have written the paper as I wrote it, I can at all events supply the criticism on what he has written.

In the first place, Mr. Reddie and our Chairman have, I think, entirely misunderstood the sense in which I used the word "credible." Allow me to quote my own words again—and really almost the whole of my reply will consist in turning back to my original words, and showing the difference between what I really said and the sense put upon it by subsequent speakers.—What, then, do I mean by the credibility of a hypothesis? Do I mean its being true? I have over and over again put the two things in as clear antithesis as it is possible to put them, and yet the Chairman says to-night that a hypothesis can only be credible if true. (No, no.) But I say a hypothesis may be credible even though not proved to be true. Having used the word in that sense, it is unfair to have another sense put upon it, in order to make it appear as if I had not proved what I professed. What I said was this: "Before any hypothesis can be admitted as certainly true, it must satisfy all these four requirements. Until it does so, it can only be accounted as more or less credible." I therefore distinctly use the term as applicable to a hypothesis *before* it is proved to be true—

The CHAIRMAN.—I have argued against the credibility of Darwinism. But I no more felt bound to accept your definition of credibility than of species.

Mr. WARINGTON.—Precisely so; I am only disputing your right of taking away *my* meaning from what I wrote, and putting your own in its place. Let us read what Mr. Reddie says about this matter. I have said at the end of my paper that it would be very rash to come to any definite conclusion until the facts of the case are better known; but in the meanwhile I submit that the hypothesis is "certainly credible." This Mr. Reddie censures as "jumping to a conclusion." Surely he must give me credit for a little more sense than to suppose I should thus contradict myself within a couple of sentences. I pass from this to the next point—the philosophic principles upon which I went to work. It is said I have been in love with hypothesis, and altogether ignored the Baconian system of philosophy. What have I really said? I have said that hypotheses are useful things for a man to keep in his mind. I have said that I believe "practically" hardly any discoveries have been fully made out without the discoverer having a hypothesis upon which he was working. For example: no one who looks at the discovery, whether true or not, of universal gravitation, can dispute that Newton went to its mathematical demonstration with the hypothesis already in his mind; and if he had not had the hypothesis, it is certain he would never have attempted the mathematical demonstration. That is exactly the practical use that I submit hypotheses have—that if there is a fair amount of *à priori* possibility in their favour, they induce men to seek for facts in order to ascertain their truth or falsity. And this is really what I said,—that a man would seek for facts, to see whether his hypothesis was true or not; never that he should go to facts in order to prove it true, for then he is an advocate. I do not say that any theory is to be maintained simply as a hobby, or that a man is to look out for all the facts in his favour, and shut his eyes against all those against him, for that is not science. But I submit that since we are in a manner obliged to use hypotheses, not being able indeed to help it, it is necessary

we should know which hypothesis in each case it is best to keep in mind ; —to ascertain which, in respect to the subject in hand, was my only object in the paper laid before you. Then as to that unlucky, I must say very unlucky, analogy of gravitation. I say very unlucky, not because I disbelieve gravitation any more than I did, but because that part of my paper has been so absolutely misunderstood. It has been dealt with as if I made a distinct comparison between gravitation and Darwinism, and used the one in some way to support the other. (Hear, hear.) What have I done ? I have done this ;—I had occasion to employ four technical terms, each capable of very large and diverse use, and I wanted every one to understand exactly what I meant by those terms. I thought, How shall I do this ? Lawyers, I believe, are finding out that it is wiser, instead of attempting to frame precise definitions, rather to give illustrations ; and so I thought I would do the same, and illustrate rather than define the meaning I assigned to these technical terms. I had in my head two illustrations equally adapted to my purpose—the one I actually used, of gravitation, and the one which Mr. Mitchell (who though he hates hypotheses, yet sometimes uses them with good effect) gave us the other evening about meteors. I distinctly thought of putting that in as my illustration. Clearly then for Mr. Reddie to go through the various points in which he thought there was a resemblance between Darwinism and gravitation was throwing time away. Let me quote the exact words I used about this illustration of gravitation. I said : “The precise meaning to be attached to these terms, and the value to be set upon the tests they denote, may be best seen by a simple example.” I pass by, therefore, all that Mr. Reddie has said about these terms as applied to gravitation. They have nothing to do with what I have said. They are interesting to those who want to go into the subject of gravitation, but they have nothing whatever to do with Darwinism. Then as to *Neptune*, what was the point I was arguing ? It was this :—It is perfectly conceivable that there may be a flaw in our evidence concerning any explanation of a phenomenon, which arises, not from a real absence of evidence, but from our being unable to get at it. Every theologian puts it down as one of the canons of criticism that whenever a possible explanation of any contradiction between passages of Scripture can be given, although you cannot prove one iota of that explanation to be true, the force of the objection is thereby destroyed, since it is shown that there *may* be, after all, no contradiction at all. I say that principle is laid down by theological advocates as one of the primary canons of criticism. I believe the canon is a true one in science also, though I do *not* say that when such flaws in the evidence exist, the hypothesis stands on the same level as before. Let me read my words again on that point. I said : “It is plain, then, that no objection to a hypothesis should be regarded as of final weight for which a possible explanation can be given not inconsistent with observed facts. Weaken the credibility of the hypothesis such objections can and do ; destroy it altogether they cannot.” And I submit, however much the thing may be laughed at, that so long as the fact remains that a planet could exist, and could roll round the sun, without being possibly visible

to us, any perturbations which could be explained on the supposition of such a planet existing would be no absolute disproof of gravitation—

The CHAIRMAN.—Except that no one has told us that such a thing is possible as a revolving planet which should not reflect light.

Mr. WARINGTON.—If you take so simple a substance as lampblack, and properly prepare it, you will find it reflects no light at all—

The CHAIRMAN.—Newton found nothing like this in his experiments.

Mr. WARINGTON.—Well, as to the total absence of reflection, I may be straining the matter too far ; but of this I feel sure, that were there a planet coated with lampblack at the distance of Neptune, it would be invisible to us, and that would answer my purpose quite as well—

The CHAIRMAN.—I should consider things like that to be among the improbabilities of nature.

Mr. WARINGTON.—Now see what has been the result of this misunderstanding about my illustration. Not only has Mr. Reddie gone out of his way to refute universal gravitation, but he has entirely missed the real object of the illustration ; and so when he comes to use the terms technically, as I used them, and in reference to my paper, it is in a totally different sense, just because he has been so absorbed in refuting, as he thought, the substance of what I said about gravitation, as entirely to overlook its true bearing on the matter in hand as an illustration of my meaning in the use of these terms. Then in Mr. Reddie's remarks about my definition of "species," there is again unfairness. Mr. Reddie writes thus, quoting my definition : "A species is a race of living beings possessing common characteristic differences from all others, which differences *at the present time are constant and inherent.*" In the next sentence he leaves out the word "differences," and tells you, "It is admitted that at the present time the characteristics of species are constant and inherent." I said nothing of the kind. Then what use does Mr. Reddie make of this? He says that the proposition I put forward is inharmonious, because my definition of species contradicts the theory. (Hear, hear.) Is there any contradiction? No. I have not said that all the characteristics of the plant or animal are constant and inherent, but that the characteristic differences of the *species* are so—a very different thing indeed. To give an illustration: Heat and light are convertible things ; no one would dispute it for an instant. You can change light into heat, or heat into light—

Mr. REDDIE.—I must dispute it. You may do it sometimes, but not always. As a rule, you *cannot*.

Mr. WARINGTON.—I simply say the thing can be done. If, now, I wanted to define heat, I should certainly put in my definition some clause excluding light ; and if I wanted to define light, I should in like manner exclude heat ; and I should say that these points constitute the characteristic differences of light and heat. I should also say they are constant and inherent—that they cannot change ; for if they change, then the heat is no longer heat, the light no longer light. (Hear, hear.) So with species : if the characteristics of species vary and are uncertain, then those characteristics thereby at once cease to be

specific. That is really all I have said about species ; and no one, I should have thought, could have imagined I meant to say that the characteristics of species regarded in themselves were invariable. Then we come to another misunderstanding. I used the term "made." Mr. Reddie quotes me, and substitutes the word "created," placing it between inverted commas, as if the exact word I used. Now I use the terms "made" and "created" in two different senses. It may be wrong to make such a distinction ; but when I say "created," I mean created in the first place, not out of anything that existed before ; and when I say "made," I mean made out of something which did exist before. I do not at all assert that the words in the Hebrew bear out the distinction, but it is good to have two words thus distinct in sense. I said, therefore, "We believe that all living things we now see about us were *made* by God." I do not say created ; and I never said God created all living things at the present time. I could not say so—

Rev. Dr. IRONS.—Forgive me, but as a matter of fact, I do not think you are fairly representing Mr. Reddie's argument on this point.

Mr. REDDIE.—As it is so late, I did not wish to interrupt Mr. Warington ; but I cannot admit any of these various interpretations of my arguments ; and I must appeal to my paper.

Mr. WARINGTON.—Then as to the question itself, whichever word we use, I want to know whether it is not as true, "philosophically," that God made *me* as that God made Adam ? If I look to the only authority we have to appeal to upon a matter of this kind—Scripture—I find David says that he was "fearfully and wonderfully made," and does not herein look to Adam, but to himself ; for if you look at the context, you find he is referring distinctly to his own individual creation or making, or whatever you call it ; and I say it is a simple truth that God made all living beings we see around us ; and how ? I say He made them (I will quote the exact words, for I do not wish to run away from what I have said) : "By means and under the influence of the causes involved in Darwinism." What means ? By reproduction—

Mr. REDDIE.—Adam ?

Mr. WARINGTON.—Do we see Adam about us ? I never said all living species, but that all living things we see at the present time have been made by reproduction, and whatever variations they have, they have got them in the same way which Darwin lays down in his theory. Now how is this phrase, that all living beings were made by God in this way—how is this understood ? It is said to be an incredible assertion, because "we *know* nothing of the transmutation of species." I never said we did *know* that existing species are the transmuted descendants of others, but simply that by inheritance and reproduction they are what they are. Carry the same principle backwards, and you have nothing more nor less than Darwinism ; and I say if production in this manner be thus in harmony with God's present mode of acting, there is no possible reason why He should not have acted in this way also in the past. Then again (I am sorry to be obliged to go into such detail, but there really is no other way of meeting the paper), we come in the next paragraph to another

misuse of my words. Mr. Reddie says: "I must exclude from my definition—again using Mr. Warington's words—'all mere transient sports or temporary variations,' as well as 'all apparent varieties dependent upon situation, climate, &c.'" I have had considerable difficulty in making out the meaning here, but as far as I can make it out,* it is that we must exclude from the definition of the causes involved in Darwinism all mere transient sports or variations from climate. That is what I understand by Mr. Reddie's remark. It is certainly not at all that which was conveyed by my words. I have simply said, because now and then a sport occurs in any species, introducing transient variation in some characteristic, that is no reason for regarding that *characteristic* as not specific; but I never said you are to exclude such things from the definition of what Darwinism is. Now we have a most extraordinary way of dealing with Darwinism. We are told "these may be Mr. Warington's deductions from Mr. Darwin's book or Mr. Darwin's own views;—but hear what Dr. Louis Büchner says." If any one else adopt Dr. Büchner's theory, it is not Darwin. When Creation is denied *in toto*, that is Büchnerism, not Darwinism. Dr. Louis Büchner, having extraordinary opinions as to what God is, comes to certain strange conclusions. Darwin has, so far as we know, no such opinions, and does not come to such conclusions—

The CHAIRMAN.—The same conclusion arrived at by Büchner—that of the self-evolving powers of nature—was, I believe, used in *Essays and Reviews*, and has been accepted by Darwin as a philosophical interpretation of his theory. I believe Darwin has never repudiated this as being a fair deduction from his own theory.

Mr. WARINGTON.—I wish to quote Darwin from his own book, fourth edition, the last sentence: "There is grandeur in this view of life, with its several powers, having been originally breathed by the Creator into a few forms or into one."—(P. 577.) Does not that settle the matter that he holds to Creation? The phrase still stands just as it did; it has not been expunged, and there is no attempt to change or modify its force. Then Mr. Reddie says Darwinism "plus Deity" may be possible. Now what is Mr. Reddie discussing? Not Darwinism "pure and simple," which is Darwinism *and* Deity; but Darwinism *minus* Deity, which is

* I think reference to my words (p. 74) will show that I am not there professing to define Darwinism in Mr. Warington's language; but that, having *bond fide* adopted his words, I am defending my own argument, where I admit the *kind* of effects Mr. Darwin lays stress upon, without admitting the extent to which he assumes they operate in nature, while he ignores those limits of Nature's laws of which we have knowledge. I say, therefore, that if we "exclude all mere *transient* sports" and "*temporary* variations," &c., we are then restricted to "characteristics" (or "characteristic differences," which I consider precisely the same in meaning), which are "constant and inherent at the present time;" and if constant and inherent *now*, so we must conclude they were "in the past;" and if so, that this is contrary to the whole theory of Darwinian modifications or changes, and "the origin of species." I hope this explanation will make my argument quite free from misunderstanding.—J. R.

a false quantity, and not what Darwin ever set forth. We come now to another misrepresentation as to the consistency of Darwinism. We are told by Mr. Reddie, "Mr. Warington thinks it the severest possible test to require that a theory should apparently agree with the facts or phenomena it has been invented expressly to account for." I have said nothing of the kind. I said that in the particular case of Darwinism this test happened to be to my mind the severest; and why? Not because this test is, as a rule, the severest, but because in the case of Darwinism the field which it covers is so enormous that it is practically the severest test. But the impression is conveyed by Mr. Reddie as if I laid it down as an axiom for all theories. Then as to geology, it is asserted that I have said the theory does not need geological evidence. I never said a syllable of the kind. We have not got such evidence complete; there is no hope of getting it complete; and it is certain that if such transformations are going on at the present time, we should not get evidence of it in the geological strata now forming. I do not say that the evidence of geology would not be an excellent test of Darwinism if we had it complete, but we have not got it thus complete, and therefore must dispense with it. (Hear, hear.) Now we come to the matter of design. I have used a phrase which has been twisted and turned all manner of ways: "A symmetry and manifest method strongly suggestive of especial design and arbitrary plan." What was I referring to? Was I referring to what we call organs of designs, *i. e.*, organs in different beings fitted to the life of those beings? No; I was referring to classification only; and I said if living beings had not been connected in the peculiarly natural manner in which they are, but in a more arbitrary manner, it would have been suggestive of especial design; we should have thought of them as having been arbitrarily marked out by some one who planned exactly where they should be. If I go, for instance, into a flower garden, with the flowers artistically arranged in rows and plots, there is evidence at once of an arbitrary plan which shows special design; but if I go to a bank of wild flowers, with all the flowers mingled anyhow, I see there no special design of the same kind, no arbitrary plan, no parting out in rows and plots: the flowers have grown naturally. I say, then, so far as classification goes, there is nothing in the connection of species with species, and group with group, which evinces arbitrary plan suggestive of special design, but rather the whole classification is purely natural. Then we have something about there being no struggle for existence at first, because there were few beings. Of course not. These beings would, however, certainly multiply, and then the struggle for existence would begin, but I never said that it always existed. Then as to orchids and insects, I never said they did not come into existence at the same time. Darwin would tell you that, upon his hypothesis, when the orchids came the insects came, because one was necessary to the other, and could not exist without it. Then we come to what is made a very grave objection, that Darwin converts the exception into the rule. Nothing of the kind. Darwin never says variation is the rule; he says, on the contrary, that he regards it as the exception. But he says this, that when variation *does* come it will very

likely be preserved, and by preservation give rise ultimately to new specific forms ; and therefore, so far as the origin of these living beings is concerned, variation has been the cause in every one of them of their peculiarities. That, however, is not saying variation was the rule—nothing of the kind. Then we are told that this variation involves inconsistency with the rule “like produces like.” What then are we to do with the descendants of these variations. We have two principles at work, one (the rule), to inherit the same peculiarities ; the other (the exception), to vary them. When, then, you have an individual plant or animal that has varied from its progenitors, what are the descendants of that plant or animal to do ? If they follow the rule, they perpetuate the variation, because “like produces like.” If they follow the exception, they revert to the original type. Therefore, on Mr. Reddie’s principle, the instant variation occurs it should be perpetuated, because “like produces like.” Then comes another extraordinary statement,—it amuses me, the number of misrepresentations and misunderstandings in this paper ! We are told that on one occasion Darwin brings in “use and disuse” because his own “peculiar theory of the struggle for existence is itself felt to be inadequate.” One would suppose from this that Darwin’s theory of the “struggle for existence” was a theory to *account* for variations. Let me read what Darwin says himself : “Several writers have misapprehended or objected to the term Natural Selection. Some have even imagined that natural selection induces variability, whereas it implies only the preservation of such variations as occur and are beneficial to the being under its conditions of life.”—(P. 91.) How can it be said, then, that bringing in “use and disuse” as a means of varying organs is helping out the theory of natural selection ? Natural selection is simply the law by which these variations, when they occur, are seized hold of and perpetuated ; nothing more. To regard it as the cause of the variations is simply to misunderstand Darwin’s theory utterly. Then as to reversion, Darwin’s pigeon, which he, after a good deal of especial pains, got to revert to the original, how is it misconstrued ! He took two pigeons which bred extremely true, such as were scarcely ever known to show a symptom of reversion ; he caused them to breed, and he obtained a mongrel. A mongrel is not a reversion. In like manner, he obtained another mongrel, put the two together, and then, and not till then, did reversion appear. And yet this is quoted as a proof—an admitted proof—that varieties *will* come back and revert to the original type—

The CHAIRMAN.—I said it was a well-observed fact among naturalists that all natural varieties which man produces have a tendency, when man’s interference is taken away, to revert to their original types. I did not think it necessary to quote Darwin for this ; but I said it was admitted in two remarkable instances by Darwin that the thing held true.

Mr. WARINGTON.—I think you were misquoting Darwin, with all due deference—

The CHAIRMAN.—You have forgotten a passage in which he says that, “*do what man will*, there is always a recurring to the original blue rock-pigeon.”

Mr. WARINGTON.—One more remark upon the paper, and that is a view

which Mr. Reddie fathers on me—yet to which I gave in reality neither birth, house, or lodging—that the new species are always progressing upwards. I do not believe it. The variations may as likely be downwards, or sideways; Darwin repeatedly repudiates the idea of there being any *necessary* tendency to pass upwards from a lower to a higher species. Now as to the later speakers. The principal point to be noticed in Captain Fishbourne's speech is his total misunderstanding of what is meant by natural selection. Let me read what Darwin has said in explanation of this much-abused term: "Others have objected that the term selection implies conscious choice in the animals which become modified; and it has even been urged that, as plants have no volition, natural selection is not applicable to them! In the literal sense of the word, no doubt, natural selection is a misnomer; but whoever objected to chemists speaking of the elective affinities of the various elements? And yet an acid cannot strictly be said to elect the base with which it will in preference combine. It has been said that I speak of natural selection as an active power or Deity; but who objects to an author speaking of the attraction of gravity as ruling the movements of the planets? Every one knows what is meant and is implied by such metaphorical expressions, and they are almost necessary for brevity. So, again, it is difficult to avoid personifying the word Nature; but I mean by Nature only the aggregate action and product of many natural laws, and by laws the sequence of events as ascertained by us."—(P. 91-2.) Now, I think in the face of this it behoves every one who wishes to speak of natural selection to understand what it means—

Captain FISHBOURNE.—But the quotation goes on, "Further, we must suppose that there is a power (natural selection) always intently watching each slight accidental alteration with the transparent layers, and carefully selecting each alteration which, under various circumstances, may in any degree tend to produce a distincter image. We must suppose each new state of the instrument to be multiplied by the million, and each to be preserved till a better be produced, and then the old ones destroyed. In living bodies, variation will cause the slightest alterations, generation will multiply them almost infinitely, and natural selection will pick out with renewing skill each improvement."

Mr. WARINGTON.—That occurs *after* the passage I have read. Surely you are bound to interpret one by the other. I will read another, page 95: "It may metaphorically be said that natural selection is daily and hourly scrutinizing, throughout the world, every variation, even the slightest—rejecting that which is bad, preserving and adding up all that is good." Darwin says distinctly that when he speaks thus he speaks metaphorically, not literally. The remarks of Mr. Manners I pass over, because to enter fully into the interpretation of the account of creation in Genesis would take quite another evening. Perhaps some day I may go into it. Then as to Dr. Thornton's objection, that there is no real struggle for existence because all forms have existence: so they have, but yet we commonly speak of a person "struggling for existence," certainly not in the sense of struggling to come

into existence. The objection of Dr. Gladstone, that every variation to be preserved must be, according to the theory, an improvement, which in many cases of intermediate stages of development presents a grave difficulty, I had meant to deal with at length, but time forbids. I am well aware that there is a difficulty here—one of the greatest, indeed, which the theory involves. (Hear, hear.) I think, however, there are fair reasons which can be brought forward to show that the difficulty is not so great as may seem at first sight ; but it is impossible now to go into this at length. Then there is the objection of Mr. Ince as to Noah's ark being too big for a few species. That supposes that the variations have all taken place since the days of Noah, which Darwin certainly does not at all suppose, but, on the contrary, asks for millions of years. Of course if you can prove that all the races of animals now living came into existence within a short period of the flood, you have a strong objection against Darwin's theory—the strongest conceivable ; but for myself I do not think that ever will or can be proved. Then as to the presumption of limiting God to the creation of a few species instead of many, I confess I cannot see it. In the same way I might say it is presumption to say God made only Adam, and did not create every man. You may as well say, when I assert that every man now living is a born man, and not created, that I am limiting the power of God—

Mr. REDDIE.—We do not consider it presumption to believe that God created only Adam, because we believe this to be revealed to us. (Hear, hear.)

Mr. WARINGTON.—I simply say that the same line of reasoning which would make it presumption to limit creation to a few instead of many species would, applied to a strictly analogous case, make it presumption to suppose God had not created all men as well as Adam. As to Mr. Mitchell, his opening remarks I am compelled to pass over this evening, and proceed at once to that other hypothesis which we are told does account for all the facts. Undoubtedly it does so ; but how ? Not by hypothesis at all. Regarded as a matter of science, it says simply we know nothing about it. It gives no scientific or natural cause for the existence of living beings at all ; it merely says we owe our origin to God, and do not, in fact, know anything about *how* we came into existence—

The CHAIRMAN.—I think you are putting this in an unfair form.

Mr. WARINGTON.—We are obliged to say, when asked how this came about, that we believe it came about in some way from God. Now we are brought, sooner or later, to this point in every science—a point beyond which our investigations cannot go, and where we must be content to refer the matter simply to God's immediate action. My position, then, is this : it does not matter in the least, theologically, *where* that point lies, near or far away. God's ultimate relation to all things is as true if it be placed at a period countless millions of ages ago, as if it be placed but at yesterday. I object, therefore, to any comparison between these two hypotheses, in the favour, theologically, of the one rather than the other, because the one takes us a little quicker to God's immediate action than the other. Then as concerns the

proof of this so-called Biblical hypothesis, whether the Bible really does teach what is alleged concerning the origin of species,—that I must pass. One word, however, as to the argument of design. Supposing Darwin's hypothesis true, what does it amount to theologically? It amounts to this: that God, having respect to the well-being of His creatures, has impressed a law upon their existence, that they shall always remain marvellously well adapted to the circumstances in which they are placed. Darwin says repeatedly that the end which is attained by natural selection is, that every creature in existence, plant or animal, shall always have by variation and competition a nature well adapted to the circumstances in which it is placed, because the best adapted will invariably be those which are preserved. It seems, therefore, to me, that it matters very little—as far as design is concerned—whether these adaptations were designed separately by God for each individual, or whether He so ordered the laws which govern life that each individual should perpetually become thus adapted. Rather does it seem to be more marvellous, more God-like, to implant one principle capable of making all individuals for all ages admirably adapted to the places in which they live than it is separately to design and fashion each. Take for illustration, an automaton draught-player whose hands are pulled by wires. For every move the automaton makes, a wire has to be pulled on purpose to make him take that move. Babbage thought he could make an automaton after a wiser fashion than that, and he made one that should choose its own moves, so that whatever move its antagonist made, the automaton immediately, and as it were of itself, took the right move in answer;—surely a far greater display of skill and design, a far higher proof of genius. So, too, it seems to me it is a greater and more marvellous thing, if God fitted all creatures to the place in which they live by means of a law impressed on a few original beings, than if He separately designed each one—

The CHAIRMAN.—That is not the view taken in Darwin's works; it is the view in the *Vestiges of Creation*, which Darwin ignores. I said design was to be sought for throughout creation. Darwin ignores design; and the passages were to show, both with regard to the formation of the eye and the instinct of the bee, it was an ignoring of the existence of design; and no fair interpretation can be put on the words of Darwin which does not include that idea, and this idea must be adopted by those who would quote Darwin as proof of the existence—of the self-existing, self-evolving powers of nature.

Mr. WARINGTON.—These passages never gave me the idea which Mr. Mitchell says they give him, and there are others also to whom they have never given such an idea. The extreme lateness of the hour renders it impossible for me to go as I should wish into the details of the argument about the eye and the bee's cell. I do not for an instant deny the wonder involved in how these things came to be, but I confess I do not see how you make it one whit more incredible if you suppose the bee to have acquired its instinct rather than had it innate. That such an insect should make such cells is a marvel in itself, but I do not see how it is a greater marvel to suppose it gradually acquired than to suppose it created; but I dare not attempt to enter into a

full explanation on this head, nor yet as to the still more marvellous structure of the eye, because time is so far gone. To my mind, neither in the least touches Darwin's position. One word, in conclusion, as to hybridism. There are cases of animals and plants which, up to the moment when they were found to yield fertile hybrids were regarded by naturalists as species, and it strikes me the same thing would be done if it were found that mules were fertile: we should at once be told that horses and asses were one species. Such reasoning can only be described as begging the question. It is asserted, because the offspring are sterile, therefore the original parents were true species. But to establish this you have to prove, as a fundamental basis, that the true test of specific difference lies in this one solitary set of organs—those of reproduction, so that those organs and those only will be the true index. Until I see that fundamental basis not *assumed*, but proved, I confess I shall object to take sterility of hybrids as a fair test of what are species and what not. In conclusion, let me say that I do not wish to be considered as pledging myself to Darwinism in any way. I do not think there is sufficient evidence in his book to prove his theory to be true; at the same time I do feel strongly—and mainly from the discussions in this Institute—that the current objections against Darwinism are invalid and fallacious. The discussion of to-night and last evening has considerably strengthened that conviction. I am no nearer believing Darwinism than before, but I am certainly more convinced than ever that the objections urged against it are irrelevant or inconclusive.

The meeting was then adjourned.

NOTE.

In the *Anthropological Review* for April last, No. 17, will be found the following account of a discussion which recently took place in the Anthropological Society of Paris, "On the Relations between the Anthropoid Apes and Man"; and which will be read with interest by the members of the VICTORIA INSTITUTE, as bearing upon Mr. Darwin's theory as applied to man's origin, in connection with the preceding discussion upon Mr. Warrington's Paper on "The Credibility of Darwinism"—more especially considering the eminence of the French Anthropologists whose views are therein expressed. The discussion appears to have been opened by Dr. PRUNER-BEY, in the course of his Address upon taking the Chair as President of the Paris Society, *vice* Dr. Gratiolet. The account proceeds as follows:—

On the Relations between the Anthropoid Apes and Man, by M. Schaafhausen of Bonn, translated by M. Pruner-Bey.—The scientific portion of M. Du Chaillu's work has been received with distrust by the learned. There

exists, nevertheless, no reason for doubting his descriptions of the mode of life of the gorilla. Some of the corrections of Mr. Reade of the remarks of Du Chaillu have no great bearing on the position which in my opinion this animal occupies in the scale of beings. But whatever may be the value of a profound knowledge of the mode of life of a gorilla, its anatomical structure shows us sufficiently the degree of his organization, and the size of his brain, upon which depends his intelligence. In this respect the distance between the gorilla and man is immense, a difference which has not been properly appreciated by Mr. Huxley. There is no doubt that in the brain of the large anthropoid apes, no essential part of the human brain is absent; but as regards volume, the difference is very remarkable. The assertion of Mr. Huxley that men, even as regards the volume of the brain, differ among themselves more than apes, is equally erroneous; an opinion which is founded upon the arbitrary employment of measurements of crania both rare and doubtful. The brain of the Australian exceeds two or three times the volume of the brain of the gorilla, whilst the brain of a European exceeds that of the Australian only by one-fifth. Another allegation of Mr. Huxley to the effect that, as regards the volume of the brain, the inferior apes differ from the superior as much as the latter differ from man, is also without scientific value, inasmuch as this author has not taken into account the incomparable difference of size of the above-mentioned simia, whilst in this respect man and the gorilla are nearly equal. This distance between man and ape must not be ignored; in fact, one glance at the cranial cavity reveals it. I think, however, that it was less in times past, or perhaps did not exist at all. The differences of volume in organised beings of the present world are only gaps produced in the chain by time. Transitional forms will, no doubt, be found still reposing in the bosom of the earth which covers palaeontological creation. Without entering into pretended developments, I shall confine myself to a single point.

In the present state of things, the distance between man and the animal increases under our own eye. Not merely the human races standing lowest in the scale, and presenting in their organisation many resemblances to animal forms are gradually becoming extinct, but the superior apes approaching nearest to man become more rare from century to century; and will, perhaps, in a few centuries have entirely disappeared. What is there illogical in the idea that thousands of years back the distance between the lowest man and the highest ape was less than at present, and that it would still lessen the more we ascend the past?

There is another circumstance, not owing to chance but to a natural law, namely, that the superior apes could only maintain themselves amidst inferior men; for on contact with civilized peoples they would long since have disappeared. The more that man advances, the more likely is he to break the links which ally him to brutes. There is another striking fact which deserves mention, namely, that the large apes of Asia and Africa differ from each other by the same characters which distinguish the men of these two continents, that is to say, in colour and the form of the cranium. Like the brachycephalic Malay, the orang is brown, and his head is round; the gorilla, on the contrary, is black and dolichocephalic, like the African negro. This approach of two different human races to different apes from the same countries, seems to me the most fatal objection, in our present state of knowledge, which might be made to the theory of the unity of the human genus.

M. GRATIOLET thought that there exists no reason for establishing an anatomical similitude between man and the gorilla. As regards the brain, the gorilla's is the lowest of the anthropoid apes, since the brain does not cover the cerebellum, by which he approaches the cynocephali. It is not in his size and strength that we must look for human characters, but in the conformation of the hands, and just in this he differs considerably from man. The

thumb is very short in the gorilla, and its muscles much reduced. The long flexor is replaced by a tendinous tract, the origin of which is lost in the tendinous sheaths of the flexors of the other fingers. It follows that the thumb has no independent movement of opposition. In the orang, though the thumb is shortened, it still is capable of an independent flexion; but this depends on a peculiar disposition which he had lately verified with M. Alix. In point of fact, the proper flexor of the thumb is entirely absent in the orang; there is not even found that tendinous tract existing in the gorilla; but by a singular contrivance, the marginal fibres of the adductor muscle of the thumb terminate in a tendon which is placed in the axis of the first terminal axis.

The fact which establishes a great relation between man and apes is, that in them the optic nerves open directly in the cerebral hemispheres, whilst in the other vertebrates these nerves reach the brain only by the intermediation of the tubercula quadrigemina. This peculiarity may explain the existence of a certain conformity in the manner in which man and ape perceive their sensations. But it does not follow that there is an identity in the nature of their intelligence; for though the senses are subservient to the operations of the intellect, it cannot be said that they produce it. Man must be placed by the side of the ape, but only as an animal. Man is a being apart, just as all other vertebrata must be separated, as they cannot be considered as having originated from each other.

M. Gratiolet added, that as a pupil of Blainville, with whom originated the idea of a series in natural history, he felt bound to state how much the ideas of his master had become modified. Where Blainville formerly recognized transitions from group to group, he, in the latter period of his life, only saw maxima and minima of realization for each group. He acknowledged an ideal series between types, but not a lineal series between all beings. It is thus impossible to invoke the opinions of Blainville for the support of theories tending to reduce to a single stock the numerous species composing the animal kingdom.

M. BROCA was of opinion that M. Gratiolet had misunderstood the ideas of M. Schaafhausen, who, far from supporting the theory of Darwin, on the contrary, commenced by refuting the opinions of Mr. Huxley. M. Schaafhausen is apparently a partisan of *animal series*, but there is no necessary connexion between this and Darwin's theory. It may be admitted that all families, genera, species, from the monade up to man, are disposed in series, and form a continuous scale, without necessarily admitting that the higher species are by a progressive evolution issued from the lower. Darwin's theory is a bold attempt to explain the existence of this series. It is the interpretation of a fact, and, whilst accepting the fact, we may reject the interpretation which was probably M. Schaafhausen's stand-point. The views communicated to the Society by M. Schaafhausen are both new and important. He shows that man is at present constantly engaged in the extermination of species which dispute his possession of the soil, and that he was so engaged in the past. We know that the superior human races tend to increase at the expense of the inferior races, some of which have disappeared within historical times, some will disappear, and others must have disappeared in the most remote periods. May, then, asks M. Schaafhausen, this destructive intervention of man not have contributed to enlarge the interval separating man from the group of anthropoid apes? He is of opinion that the interval was less originally than at present, and is less at present than it will be in times to come. The last opinion is very probable; the former is less so, for even if it were demonstrated, the question still would remain whether the intermediate types which disappeared sufficiently differed from such now limiting the two groups, sensibly to diminish the distance. At all events, the ingenious idea of M. Schaafhausen deserved serious consideration.

M. DE QUATREFAGES confirmed the remarks of M. Gratiolet touching the first ideas of Blainville on the animal series.

M. POUCHET considered that the idea of a linear series on the ensemble of the animal kingdom was now abandoned, and justly so, because there existed an impassable abyss between the vertebrates and invertebrates. But in confining ourselves to the vertebrata we may imagine a series resembling the branching off from an arborescent trunk, many of the branches representing as many extinct species being wanting. He therefore believed with Mr. Darwin that we are the remote cousins of the gorilla by the intermediation of a vertebrate, the type of which is now lost.

M. DE QUATREFAGES would not admit the ideas of Darwin as regards species, but admitted them with respect to races, which are daily formed under our own eyes.

M. SANSSEN could not allow this observation to pass without contradiction. M. Quatrefages would be much embarrassed to name one single race perfectly new.

M. QUATREFAGES replied that the number of esculent vegetables had, independent of new importations, remarkably increased since the time of Louis XIII., and he cited the sheep of Manchamp, Malnegrée, Charmoise, as examples of new races produced within a few years by the crossing of distinct races. The difference between him and M. Sanssen consisted only in the definition of the word race.

M. SANSSEN said it was quite true that he differed with M. Quatrefages as to the definition of race. In his opinion race is a group of individuals presenting an ensemble of similar forms and capable of being transmitted; homogeneity of typical character, and hereditary transmission, being the necessary attribute of race. And here he must remark that the term of race had not yet been defined in the Society, and an understanding on that subject became requisite. As regards the examples invoked by M. Quatrefages, they cannot be considered as new races, the sheep of Manchamp are Merinos differing only from the mother race by their silky wool. This is not a race character, the same wool being found in perfectly distinct races. As to the sheep of the Charmoise, he could show him two distinct types. They only resemble each other by their aptitude for fattening, which is not a race character. And as regards some esculent vegetables they had become so by culture. When they are left to nature their characters disappear, which does not prove that they constitute new races.

ORDINARY MEETING, APRIL 15, 1867.

THE REV. WALTER MITCHELL, VICE-PRESIDENT, IN THE CHAIR.

The minutes of the previous Meeting having been read and confirmed, the following Paper was then read :—

ON UTILITARIANISM. *By* JAMES REDDIE, ESQ., *Hon. Sec. Vict. Inst.*

THE theory of Utilitarianism could have had no more able champion in our day than Mr. John Stuart Mill, and yet some of his most favourable critics have observed that he has rather apologized for it, and explained away its most objectionable features, than ventured upon its rigid vindication. Mr. Mill would probably not admit this description of his treatise to be correct ; but, as a frank opponent of the theory, I can only say that I trust that those who have any doubts upon the subject will read and carefully study the book for themselves. I do not anticipate that it will make any converts to Utilitarianism. It will be found very full of startling propositions ; and its admissions and qualifications will most probably drive most of its readers to the conclusion that some more simple and intelligible “ foundation of morality ” is requisite than “ the utilitarian or happiness theory.” Mr. Mill believes, however, that it is the very imperfect notion which people have of the utilitarian formula that is the chief obstacle which impedes its reception ; and he commences his explanation of “ what utilitarianism is ” by exposing “ the ignorant blunder of supposing that those who stand up for utility as the test of right and wrong, use the term in that restricted and merely colloquial sense in which utility is opposed to pleasure.” He says, however, that the philosophical opponents of utilitarianism are incapable of “ so absurd a misconception,” and adds that “ those who know anything about the matter are aware that every writer, from Epicurus to Bentham, who maintained the theory of utility, meant by it, not something to be contradis-

tinguished from pleasure, *but pleasure itself, together with exemption from pain.*" And "yet (he goes on) the common herd, including the herd of writers, not only in newspapers and periodicals, but in books of weight and pretension, are perpetually falling into this shallow mistake." Mr. Mill uses this rather strong language, although he admits that the term has been thus "ignorantly misapplied," not "solely in disparagement, but occasionally in compliment, as though it implied superiority to frivolity, and the mere pleasures of the moment." Now, as Mr. Mill claims to have been "the first person who brought the word utilitarian into use," he is, of course, well entitled to explain what he may have meant by it; but it does seem somewhat unreasonable to be angry that the term has been so generally understood in its obvious sense, as signifying that which is antithetical to "pleasure in some of its forms"—to beauty, ornament, or amusement. We find the word described as a "modern barbarism" in some of our dictionaries; and our lexicographers seem all unaware that by the useful is meant also the ornamental, beautiful, pleasant, and amusing. But whether the etymon of the term be regarded as the English word *utility*, or the Latin word *utilitas*, I am equally unable to see upon what philological ground Mr. Mill can claim to be so very right in the peculiar sense he applies to it, and "the herd of writers in newspapers and periodicals," and even "in books of weight," so very wrong and "shallow, mistaken and ignorant." If, again, there was really no difference between what Epicurus and Bentham taught, "the common herd" may be excused for thinking that it might have been quite as well not to have given a new name, and one so liable to be misunderstood, to an old and well-known system of heathen morals. Not that I can admit that Benthamism and the Epicurean philosophy, are really alike, though Mr. Mill seems to say as much; any more than I think it certain that Epicurus would have rejected a higher foundation and sanction for his system of happiness (based as it was upon virtuous action alone), such as the revelation of God's will affords, if he had only had the opportunity of knowing it as we have. We must not forget that what may have been an admirable theory of morals for the heathen, and for them a sound foundation for human virtue and goodness, may have a very different character when it is professedly put forward in antagonism with Christianity. It is one thing to reject, as Epicurus did, the heathen superstitions as to a future life, and quite another to reject what the Bible and Christianity teach as to future rewards and punishments. The best of the heathen philosophers, moreover, admitted the imperfections of their own moral

systems, and the need for some higher teaching and further light to satisfy their longings after the true, the good, the holy. Those who believe in the Bible and Christianity believe they have that higher teaching and light which the heathen wanted. They ought not, therefore, to be satisfied with any theory of being or living, or any foundation of morality, which coolly ignores, and requires them to ignore and disregard, what Christianity teaches. In this point of view, and logically so, what is not founded on Christianity is against it; though at the same time we may be glad to find adduced, however faintly, among other arguments in favour of Utilitarianism, that it is not at issue with certain recognized Christian principles, and that it is, therefore, so far not against Christianity.

The real fact is, that Utilitarianism is an inadequate theory of morality, rather than a positively and altogether false one. As far as there is truth in it, it is perfectly in accordance with Christianity; and, indeed, most modern spurious systems make very free use of principles, of which but for Christianity they would have had no knowledge. But when Utilitarianism claims to be a satisfactory foundation for a moral system, and of itself capable of being a test of right and wrong, and the means of ascertaining what is right or wrong, it puts forth pretensions to which it has not the slightest right. We shall find, moreover, that the same confusion of ideas which, it seems is connected with its very name, runs through all the arguments on which it professes to be based, even when they are employed by such an able advocate as Mr. J. S. Mill; and, if so, it will be evident that it can but have slight pretensions to be dignified with the title of "a philosophical theory."

Mr. Mill gives the following tolerably full definition of his professed faith. He says: "The creed which accepts as the foundation of morals, Utility, or the Greatest Happiness Principle, holds that actions are right in proportion as they tend to promote happiness; wrong, as they tend to produce the reverse of happiness. By happiness is intended pleasure, and the absence of pain; by unhappiness, pain, and the privation of pleasure." But, notwithstanding these postulata, we find Mr. Mill thus expressing himself in another place: "The medical art is proved to be good by its conducing to health; but how is it possible to prove that health is good?" This will certainly puzzle ordinary readers, who would naturally reverse the proposition, and say they have no difficulty in proving health to be good, but it often appears to them more than questionable whether the medical art really does conduce to health. That it ought to do so, and aims at doing so, all may admit; but that is not Mr. Mill's proposition. A first diffi-

culty, also, might well be raised as to which of the various medical arts is to be regarded as *the* "medical art" that Mr. Mill calls good as being conducive to health. And what are we to think of a theory of morals which is founded upon our knowledge of what conduces to happiness, or what is, therefore, good, if at the outset we are in doubt whether we can prove that health is good? Surely, if by happiness and pleasure is meant the absence of pain, there can be no difficulty in proving health to be good, unless it be alleged that what conduces to happiness and pleasure is not good. And if, as Mr. Mill says, the absence of pain is one primary meaning of happiness, one would imagine that nothing could more logically follow than that health is good as conducive to the absence of pain, and therefore to pleasure or happiness, according to this definition or major proposition.

Side by side with this he places the following, which may be regarded as striking at the very root of the theory of utilitarianism itself: "The art of music is good, for the reason, among others, that it produces pleasure; *but what proof is it possible to give that pleasure is good?*" I venture to think that if a prior question, "What is good?" had been determined, these other inquiries would have been more logically and satisfactorily answered. It is not, however, my business to do this; and, indeed, most of the abstract questions of this kind, as raised by Mr. Mill, are much more fully and satisfactorily answered in the Dialogues of Plato than in his Essay. But, admitting that it is impossible to prove that pleasure is good, what then becomes of a theory which is professedly based upon pleasure as its grand criterion? I mean, if pleasure cannot be proved to be good, can a theory of moral action based upon the production of pleasure, as its test and foundation, be proved to be good? And if not proved, are we really expected to be satisfied with this theory of morals, which rejects the principles of Christianity, on a mere assertion that it is good, and to accept it in blind faith, without any proof whatever? We are. This is precisely what Mr. Mill demands of us in the very next sentence. "If, then," he says, "*it is asserted that there is a comprehensive formula, including all things which are in themselves good, and that whatever else is good is not so as an end, but as a mean, the formula may be accepted or rejected, but is not a subject of what is commonly understood by proof.*" I have no wish to misrepresent the claims of Utilitarianism, and I therefore add, that the above extraordinary dictum is afterwards qualified thus: "The subject is within the cognizance of the rational faculty, and neither does that faculty deal with it solely in the way of

intuition." By "rational faculty" the sense requires us here to understand *reasoning* faculty; and then the latter clause of the sentence becomes very strangely superfluous. The succeeding sentence completes the confusion and self-contradictions which seem to be intimately associated with all that relates to the nature as well as the name of utilitarianism. It is not susceptible of proof, but yet it seems that it may, after a fashion, be proved; for he adds: "*Considerations* may be presented *capable of determining the intellect* either to give or withhold its assent to the doctrine; and this is *equivalent to proof*." I quite agree with this last position; and it is solely because I consider that arguments, capable of determining the intellect, may be adduced for and *against* Utilitarianism as a moral theory, that I discuss the subject at all.

In order to make my argument as intelligible as possible, I will at once state the propositions I think may be established against the theory. First, that (as already said) it is an inadequate theory, and this in two senses—inadequate as not being practicable as a principle of action, and inadequate as not being a whole truth. Second, that whether utility (in the proper sense) or pleasure, or both, be regarded as the basis of the theory, then, in so far as either utility or pleasure, or both, can be regarded as good, so far are they recognized as good in the Christian system. Third, that therefore, so far as Utilitarianism is good or true, it belongs to Christianity. And lastly, that wherever utility or pleasure, or both, are made a motive of moral action beyond what Christianity sanctions, they will mislead, and are false principles.

Let us now have as plainly before our minds the claims of Utilitarianism. "The theory of life on which this theory of morality is grounded" is as follows, namely, "that pleasure, and freedom from pain, are the only things desirable as ends; and that all desirable things (which are as numerous in the utilitarian as in any other scheme) are desirable either for the pleasure inherent in themselves, or as means to the promotion of pleasure and the prevention of pain." This theory being announced in these terms, it is then spoken of as if it were identical with the system of Epicurus; though afterwards Mr. Mill adds: "I do not, indeed, consider the Epicureans to have been by any means faultless in drawing out their scheme of consequences from the utilitarian principle. *To do this in any sufficient manner, many Stoic as well as Christian elements require to be included.*" Here, again, we have an important concession that is fatal to Utilitarianism as a moral theory; or, at least, which requires certain important principles to be

previously accepted, if they cannot be shown to be false; and which, if true, really settle the main question, and leave no place for a theory of Utilitarianism. These principles are, That God is the moral governor of the world, and that reason and conscience are the voice of God in man's heart, and enable us to discover or to know intuitively what is right, or in accordance with God's will. Zeno taught this so far, according to the light of nature; Christ added the light of revelation of the Truth and will of God. God is the basis of the Stoical system, as it is of Christianity; but Utilitarianism is essentially atheistic. And yet it now confessedly requires to borrow "many Stoic as well as Christian elements," before it can claim to draw out its scheme of consequences "in any sufficient manner." In other words, modern Utilitarianism may now be defined as Epicureanism *plus* some of the elements of Theism, to be found in Stoicism and Christianity. But, then, I must point out that these incorporated elements are heterogeneous to the theory that adopts them. The moment the idea of God is entertained, as the author of created existence, His will must necessarily override and supersede all other considerations as the proper and only true basis of morality. This is so, whether that will is only known or sought after by the aid of natural reason and conscience, or whether a fuller knowledge of it is further revealed to man by the Scriptures. Moreover, professing Christians have a right to demand of any teacher of new moral theories—and especially of one who admits the necessity of certain Christian elements to complete the theory he propounds—that he will plainly tell them what other Christian elements the advocates of Utilitarianism are prepared to show should be set aside as false. Christians cannot be content to be merely told that "Utility is not only not a godless doctrine, but more profoundly religious than any other;" nor satisfied to learn that "the Utilitarian who believes in the perfect goodness and wisdom of God, necessarily believes that whatever God has thought fit to reveal on the subject of morals, must fulfil the requirements of utility in a supreme degree." All this the Christian himself believes, but also something more than this. Let us at least raise perfectly clear issues in all such discussions, and begin at the beginning logically. Christianity is a long-established system, which claims to be wholly true. Those who reject it, or set it aside, are bound to attack it seriously, if they have anything better to teach. They have no right to appropriate some of its "elements" to bolster up an adverse system, in order to make the latter palatable to those whose minds have been elevated, however unconsciously,

far beyond Epicureanism, or even Stoicism, solely by means of the teaching of Christianity.

We may well be surprised to find a writer, having the high reputation of Mr. J. S. Mill as a logician and reasoner, making use of an illustration by way of argument, which is no better than the vulgar *tu quoque* fallacy, which can only be answered by the common proverb that "two blacks cannot make a white." Ignoring Christianity as the lamp of moral truth, he admits there are endless difficulties, confusion, and little progress yet made among mankind in the decision of the controversy respecting the criterion of right and wrong; in short, he acknowledges, among those who thus reject the Christian rule, a condition of "ever learning, but never being able to come to the knowledge of the truth." But he pleads that "similar confusion and uncertainty, and in some cases similar discordance, exist respecting the first principles of all sciences, not excepting that which is deemed the most certain of them, mathematics;" and he says that this is so "without much impairing, generally indeed without impairing at all, the trustworthiness of the conclusions of these sciences." "Were it not so," he goes on, "there would be no science more precarious, or whose conclusions were more insufficiently made out, than algebra, which derives none of its certainty from what are commonly taught to learners as its elements, since these, as laid down by some of its most eminent teachers, are as full of fictions as English law, and of mysteries as theology."

All this may be very true, and may afford a very good reason for being on our guard against the irrational mysteries of modern analytical mathematics; but it should be remembered that these corruptions and contradictions and mysteries in pure mathematics, have crept gradually into the science, and not without protest on the part of honest thinkers. But such a description of algebra would scarcely be given by any one who accepts its methods as trustworthy. And such a bad example of credulity in a science which is admitted to be full of contradictions and insufficiently proved conclusions, affords no reason why men should reject the plain teaching of Christianity, in order to adopt a system which its very author (as we may concede Mr. Mill to be) confesses to be thus full of difficulties and contradictions. But to do justice to the analogy before us; contradictory, confused and mysterious as Mr. Mill admits modern mathematics to be, what would he think of a philosopher who, in opposing their conclusions, wished all their teaching to be quietly ignored, instead of attacking their main principles by strictly mathematical reasoning, and

so proving their alleged contradictions to be really such, or else disproving the conclusions by a *reductio ad absurdum*? What, as Christians, we must wish to know, is, Why we are to give up Christianity as the best rule of morals, and to have recourse to Utilitarianism? But in the meantime, waiving this point, and content with having stated it, let us follow Mr. Mill briefly in his advocacy of Utilitarianism upon its merits.

It claims to be "the Greatest Happiness Principle." But then does not the Christian system, proclaiming "Peace on earth and goodwill to men"—or as some translate the phrase, "Peace to men of good will"—put forward a prior claim to having enunciated "the greatest happiness principle"? Utilitarianism claims that its "ultimate end, with reference to and for the sake of which all other things are desirable (whether we are considering our own good or that of other people), is an existence exempt as far as possible from pain, and as rich as possible in enjoyments, both in point of quantity and quality." And what, pray, is the teaching of the whole Bible, Old Testament and New, and what the main thesis of Bishop Butler's *Analogy of Natural and Revealed Religion*, but this? In the Old Testament we have blessing and cursing, or happiness and the contrary, put before men as the great "end" of true morality or obedience to God's will. The paths of the just or wise or virtuous are described as paths of pleasantness and peace; and again, as "the ultimate end," it is declared that "at God's right hand there are pleasures for evermore." And we have the simple principle, "Love God first, and thy neighbour as thyself," coupled in the New Testament with the equally simple yet comprehensive rule of action, "Whatsoever ye would that men should do unto you, do ye even so unto them, for this is the law and the prophets." But the law and the prophets and the gospel, from the first to the last book of revelation, are also full of "the Greatest Happiness principle," culminating in the description of the joys of heaven in the Apocalypse, where there shall be no more death, where sorrow and sighing shall flee away, and where all tears shall be wiped from all eyes. And the means to this end are love to God and man, as principles, and the practice of universal benevolence, including justice, mercy, kindness, and whatsoever things are lovely or of good report.

Now, why is this to be rejected? We find nothing superior or at all equal to it in the Greatest Happiness Principle of Utilitarianism. It we also find to be defective as regards its foundation, since it "cannot prove pleasure to be good;" and in fact its author almost admits that some pleasures are not

good. He supplies us with no simple rules for moral action, like the Ten Commandments of Moses, or the Christian precept to do unto others as we would be done by. He only promises at best, hereafter, to set forth a code of morals.

Mr. Mill does not believe that we have any innate moral feelings; but says he thinks that if "once the general happiness were recognized as the ethical standard, this would constitute the strength of the utilitarian morality." If, however, we consider for how long the doctrine of rewards and punishments has been taught in the world, and we may say, how largely it has been admitted into the human conscience under almost every system of religion, and especially if we have regard to the promises and precepts of love and benevolence in the Old and New Testament, and the millions who have really believed in them, without acting consistently with their professed belief, we may well conclude that this laudable utilitarian hope is also somewhat Utopian. In another place Mr. Mill speaks of "the comparatively early state of human advancement in which we now live." I know not whether he accepts the old-fashioned Bible genealogy of mankind, or the new theories of man's much greater antiquity. But alas! for man's progress and ultimate end, if either six thousand years, or twice as many millions, must be regarded as "an *early* state of human advancement"! *

The practical difficulties which mankind at large would experience, had they no other moral guide than Utilitarianism, would consist in their never being certain whether this or that act would conduce to the *greatest happiness* or not. No higher motive or basis being recognised, self-denying virtue and the suffering of temporary pain, or refraining from immediate pleasure, for the sake of ourselves or others ultimately, would be impossible. Why one man should suffer for the sake of others' happiness; or how an individual could satisfy himself that he should be that man; may be regarded as inevitable puzzles that would arise under a system which has no higher or simpler standard of right and wrong. Mr. Mill thinks these difficulties could easily be got over by utilitarian precepts which might be propounded for men's guidance. But he does not propound them. When he does, I have little doubt we shall find, that many of "the elements of Christianity" must needs be incorporated with his new code of morality. Notwithstanding his definition of the Greatest Happiness Principle, however, it is satisfactory to learn that Utilitarians "do desire things which, *in common language*, are decidedly distinguished from happiness." As any system of morality in my opinion ought certainly to be suited to mankind generally,

I am therefore persuaded it must have regard to "common language" in order to be intelligible. We may hail, therefore, with great satisfaction the further announcement that Utilitarians "desire, for example, virtue, and the absence of vice, no less really than pleasure and the absence of pain." All this is very hopeful; as also is the plain admission that all pleasures are not real pleasures, or conducive to happiness. After all, "the lovers of pleasure" which throng the crowded circles of "vanity fair" will find little to please them in Mr. Mill's essay. The moment a qualification becomes necessary as to *what* pleasures conduce to real happiness, the definition of utilitarianism shows itself imperfect. These words, virtue and vice, are like the small end of a wedge of truth, and once admitted and pondered and fairly understood, only require to be driven home and logically applied, in order to make an end of Utilitarianism. The moment mankind is furnished with a higher motive than "Pleasure," or "the Greatest Happiness Principle;" and when words like virtue and vice, good and bad, are introduced as ideas (which are intelligible enough under the Christian system, as well as under that of the Stoics and the systems of all theistical moral teachers), the Theory of Utilitarianism falls to the ground, and its very name remains but a "modern barbarism," as defined in some of our dictionaries.

* Here I might stop; but before I conclude I am anxious to show, by some additional extracts from Mr. Mill's book, how the teaching of Christianity can be coolly appropriated by moral theorists, whose object is to substitute something else for Christianity; and who, it seems, can get on satisfactorily, and, as they think, produce "the power and efficacy of a religion," even "without the aid of belief in a Providence"! Mr. Mill says:—"The desire to be in unity with our fellow-creatures is already a powerful principle in human nature, and happily one of those which tend to become stronger, even without express inculcation, from the influences of advancing civilization." It pleases him, it will be observed, to ignore the fact that, even if the world is still in a "comparatively early state of advancement," there has, nevertheless, been time enough within "the historical period" for various developments of civilization to take place, but which never did happen to develop into "a desire to be in unity with our fellow-creatures," till "that powerful principle" was enunciated to human nature as the express inculcation and teaching of the religion of Christ. Again, our author says:—"In an improving state of the human mind, the influences are constantly on the increase, which tend to generate in each indi-

vidual a feeling of unity with all the rest; which, if perfect, would make him never think of, or desire, any beneficial condition for himself, in the benefits of which they are not included. If we now suppose *this feeling of unity to be taught as a religion*, and the whole force of education, of institutions, and of opinion, directed, as it once was in the case of religion, to make every person grow up from infancy surrounded on all sides both by the profession and practice of it, I think that no one who can realize this conception will feel any misgiving about the sufficiency of the ultimate sanction for the Happiness morality." I really know not what to call this kind of thing. We know what plagiarism is, and literary piracy. But what name can we give to a "moral" system that seizes upon the grand distinctive principle and peculiar characteristic of Christianity, and puts it forth as a new thing, to be "*now taught as a religion*," in order to prove the sufficiency "of the Happiness morality," which goes by the name of Utilitarianism! What follows is, if possible, yet more startling:—"To any ethical student who finds the realization difficult, I recommend, as a means of facilitating it, the second of M. Comte's two principal works, the *Traité de Politique Positive*. I entertain the strongest objections to the system of politics and morals set forth in that treatise; but I think it has superabundantly shown the possibility of giving to the service of humanity, *even without the aid of belief in a Providence*, both the psychological power and the social efficacy of a religion, making it take hold of human life, and colour all thought, feeling, and action, in a manner of which the greatest ascendancy ever exercised by any religion may be but a type and foretaste, and of which the danger is, not that it should be insufficient, but that it should be so excessive as to interfere unduly with human freedom and individuality."

I need not comment upon this. I can, however, fully agree with Mr. Mill as to the probable effect of "a religion" that dispenses with a God! When, also, any system full of "contradictions, fallacies, and insufficiently-proved conclusions," comes to be credulously accepted by men, their real freedom and individuality must not only be unduly interfered with and ultimately perish; but in my opinion they must already have become mental slaves, and have ceased to be independent thinkers.

REV. DR. THORNTON.—I shall take the liberty of offering a few remarks upon the paper of Mr. Reddie; not that I presume to add anything of my own, but as a mere appendix, I think one may be able to say something which may confirm those conclusions to which he has come, to the satisfaction

of all members of this Institute. As he has told us, Mr. Mill is not the first Utilitarian. The Utilitarian theory was known long before his time. In fact, the first Utilitarians arose at Athens, and were called Sophists. They were persons who, seeing doubt and difficulty pervading men's minds as to right and wrong, laid down this easy and intelligible rule, that what appeared right was right, what appeared pleasant was pleasant, and what a man would like to do he was bound to do and ought to do. Against this pernicious doctrine the great man of his age, Socrates, set himself most decidedly. In his disputations (those disputations which have gained him the name of "the Prince of Bores"!) he maintained that good was not what appeared to a man to be good, or rather could not be tested by each individual man's opinion of it; but that the chief good must contain three elements:—1. Intellectual truth; 2. Moral excellence; and 3. An element commending it to the feelings of those who possessed it, by means of what (for want of a better word) he called pleasure. Socrates was followed by disciples having minds differing from his and from one another's, each of whom caught hold of some one of those elements of good and maintained it exclusively. With those who maintained moral excellence to be the chief good, who were the Cynics, and afterwards the Stoics, we have nothing to do; nor with those who, with Plato, considered a highly-trained and developed intellect to be the chief good. Then we have the third school of *Aristippus*; and he maintained that what was pleasant was good, and what was not pleasant was not good. He was a Utilitarian very different to the Sophists, but he was one still: he did not perceive the logical fault he was committing in making pleasure and good coextensive with one another. Socrates had said there were three elements required in good. He required only one, and fell into the same error which the Sophists had committed; so that, though professing to be a follower of Socrates, he came to the same conclusion as those whom Socrates combated. His fallacy was this: laying down that "All that is good is pleasant"—which is true,—he simply converted the proposition, and said "All that is pleasant is good," which of course every logician knows to be incorrect. The fact is, that the pleasure is a test of the presence of good, but the goodness does not depend on the pleasantness. Granted that a certain thing is good, there must be a certain pleasure; but you must not therefore argue, because pleasure attends a course of action, that course is necessarily good. It would be a fallacy, and it is that fallacy which the Utilitarians fall into now, when they say the test of the goodness of an action is its producing pleasure, or freedom from pain, amongst the greatest number. Now this question is a very important one, because it leads to still further considerations, to what I may call the boundless realm of moral obligations and moral sanctions. (Hear, hear.) Why are we bound to act in a certain way, and not to act in another certain way? Why has there been a certain stamp fixed upon a certain course of actions by which the Deity says, This shall not be done; That course shall be adopted? The whole question is one of difficulty; but the Utilitarians, it seems to me, appear to

try to do away with it. Every one who considers the matter dispassionately, and thinks of it as deeply as thinking men should, will say that the subject is one of boundless extent. The Utilitarians, however, endeavour to narrow it by saying, "We have an easy test of goodness and propriety of actions; they are obligations upon us, and their sanction is tested by the pleasure, positive or negative, produced by them in certain individuals." Into this subject I do not now profess or wish to go. It is one which would occupy much more time than at present we have to spare. I shall, however, endeavour to point out one thing, that the Utilitarians have neglected in their theory (their theory of goodness and pleasure being equipollent and coexistent), namely, the true consideration of what causes the pleasure of good actions. Why is a good action pleasant, and why is an evil action unpleasant? Because of a faculty which we call moral taste. As the moral sense is the intuitive perception of that which is considered moral good, so the taste is the intuitive perception of that which is beautiful. Moral taste, then, is the intuitive perception, not of the goodness, but of the beauty and fitness of virtue. That is the faculty which Utilitarians ignore, by making goodness and pleasure equal to one another, and each a test of the other. They have forgotten there is this faculty of moral taste; or they confound it with the conscience, or moral sense. Is this faculty implanted in us, or is it one gained by training? If we look to our Scriptural guide (and that is a very safe one,—it is a good philosophical book, as well as our guide for affairs of higher concern), we shall find what philosophy would have already taught us, that the moral taste is something gained by training and by experience; the faculty, the power of perceiving the beauty of virtue and goodness, arises from the education of the man by obedience to his moral sense. One who habitually follows the dictates of conscience will arrive at a state of mind in which he will intuitively perceive that a virtuous mode of action is not only the one he is bound to adopt, but the most delightful to adopt; he will perceive the pleasure connected with virtue: but that state of mind does not come until after the mind has been trained. We find it in Scripture in these words, "If any man will do His will, he shall know of the doctrine." We shall have not the mere discerning that good is not evil, and evil not good, but more than that: we shall have the moral taste exercised to perceive that good is in itself essentially beautiful, and evil in itself the reverse; that goodness, if we may say so, shows, even on earth, some reflex of the bright face of that Deity whose will we believe it to be. (Hear, hear.)

MR. R. G. M. BROWNE.—One thing occurs to me, that Dr. Thornton has referred to Scripture in support of his assertion that moral taste enables us to judge of the beauty of goodness. Should we not rather discuss the theory which Mr. Mill would put forward on its merits, independently of Scripture? That is a point which it occurred to me might be regarded by some as rather a weakness in the argument; and whether Utilitarianism should not be considered independently of Scripture. I think Dr. Thornton quoted from Scripture in support of his assertion.

DR. THORNTON.—Far from it; I wish you to understand distinctly that I

do not refer to Scripture as a proof of philosophy, I merely say (perhaps I failed to express myself clearly) that the philosophical principle is, that there is a moral taste or perception of the beauty of the goodness of virtue, distinct from its fitness ; and this, I said, I cannot express better than in the words of Scripture,—for I believe Scripture to be a book which contains true philosophy, as well as guidance on higher subjects ; and therefore I rather use the words of Scripture, as the best words to express my philosophical principle, than found my argument upon them. I may apologize for using words of Scripture, which occur naturally to a person of my profession ; but I wish it to be understood that I do not appeal to Scripture as a proof of science ; but I do think that the words of Scripture express the scientific truth so well, that I may be pardoned for employing them. (Hear, hear.)

Rev. A. DE LA MARE.—I am unwilling to put myself before the meeting this evening, though I cannot refrain from making a few observations. As you are aware, I have not been able to attend the meetings for some time, and I may be a little behind the course of lectures which have been delivered. I would first say, I thank Mr. Reddie most sincerely and heartily for myself, for the paper he has read to us. It has opened an important question, which I think might be very usefully discussed at some future time, and would likely bring forth a rejoinder or awaken a desire to pursue the subject further. The remark of the gentleman who just now demurred to Dr. Thornton's quotation of Scripture suggested to my mind, that if philosophers would adhere as strictly to their own definitions of their own systems as they require theological students to do, we should have less trouble in understanding each other, and in keeping separate truths which they are prone to amalgamate. We have often high claims put forth for different branches of science and systems of philosophy, which men choose to introduce as "if they were new things under the sun ; but if we look to it, I think we shall find, as Mr. Reddie says to-night, that all the real good in their systems, from beginning to end, is to be traced from that one source from which Dr. Thornton has quoted, and for doing which I thank him. With regard to the subject which has been brought before us to-night, it is one which has occupied perhaps some of the most acute intellects of the day, and one which requires a great deal of reflection before speaking upon it 'in public. I would not, therefore, attempt to discuss the question, I only feel most distinctly and decidedly, that Utilitarianism involves a wrong principle, inasmuch as it does not go upon the system of right and wrong. Mr. Reddie has brought before us how its advocates are beginning to introduce the terms of virtue and vice ; but when that is the case, I do not see how they can stop short of introducing the principles of right and wrong ; and when that is done, and virtue and vice are treated in their real characters, they are reducing Utilitarianism to Christianity. I must excuse myself for intruding upon the meeting these few remarks ; but as this is the first time I have been able to be present in the Institute after a long absence, I felt desirous of stating my feelings with reference to the subject before us. (Hear, hear.)

Captain FISHBOURNE.—I may observe with reference to that objection to Dr. Thornton's remarks, that the paper distinctly mentioned as one of the defects in the theory, acknowledged by Mr. Mill himself, that it requires to draw from Christianity; and, if he does that, he must admit the propriety of any opponent appealing to that system he draws from. With respect to those attacks made on Christianity, they have one general characteristic,—the persons who attack it evince the most profound ignorance as to what Christianity is; and when they are shown to be wrong, they give a different interpretation to their expressions, in order to get out of the difficulty, and say that "We do not understand them." Now we can fairly retort, and can make it obvious to any one, that they are essentially wrong, and do not really understand Christianity. To take one illustration that is patent to all, in which this system that Mr. Mill proposes, of making happiness a test, is utterly wrong and false. Happiness, he says, is a test of what is right. Why, the whole moral government of God is impeached by this. The whole of the physical difficulties, the physical suffering in the world, is all remedial, and although all painful, is intended to be good in its issue. If we go into a hospital, I could show him there the result of Christian principle as a fact—deal with it as he will.—I would take him in there, and we should see persons under the power of Christian principle, raising them altogether above the sense of pain to a certain extent; so much so that they would not be without pain, because of the superabundant enjoyment which they get from the realization of that Divine power and presence which accompanies the pain, and lifts them above themselves and surrounding circumstances. (Hear, hear.) You will find Christians at the present moment at the East End of London, and I could show there persons without a single outward condition that constitutes, in his estimate, happiness, yet enjoying all the feelings of happiness; and they would despise anything he could offer as a substitute for the condition in which they are living, though bereft of everything—of all the conditions which he would say are indispensable for happiness. He breaks down in these cases in every phase. I think we must congratulate Mr. Reddie upon the not only dispassionate, but I would say far more than dispassionate tone, towards Mr. Mill; for Mr. Reddie really gives him credit where he does not deserve it. Here is a passage, for instance, in which he has given him credit for Christian principle; but I do not think Mr. Mill understands it himself,—“The feeling of unity to be taught as a religion.” But how is the feeling to exist unless the unity has taken place? How can there be the feeling of unity if there is not unity? Then there is a power in Christianity which produces unity, which no mere human system can do. It is a Divine power; a Christian united to Christ is united to God, united to all Christians; but there is no human system that can produce that. Now, all that is overlooked; there is nothing to contrast with it, and yet Mr. Mill's ignorance of this induces him to set forward his system or theory, because he is utterly ignorant of what Christianity is,—because he won't see what evidences there are to be seen by any man in any country like this where Christianity is preached.

Mr. BROWNE.—It was by no means with any feeling of opposition to the Scriptures that I ventured to make the remark I did, for I am a thorough believer in them, but it was on account of my jealousy for them. I think they are maintainable on independent grounds, but I thought it was quite right, when an observation was made that seemed to be somewhat dependent on Scripture, to say that the point was by no means dependent upon the Scriptures.

Captain FISHBOURNE.—I hope you do not mean I fancied it was ; but I thought it was a fair answer to be urged, that Mr. Mill had given the warrant for the introduction of the Scriptures by himself borrowing from the Scriptures.

The CHAIRMAN.—I feel that anything I can possibly say, after what Mr. Reddie has written upon this subject, and has been so ably followed by Dr. Thornton, would be only weakening the argument. I think this is one of those subjects which only require to be brought before men who know something of Christianity, in order to see how worthless that philosophy must be, and that it is essentially atheistical. It comes before us as atheism, inasmuch as it is an attempted foundation of a morality without a God, and without recognizing the existence of a moral principle, or the admission of anything like moral principle. Those who have advocated this system in ancient or modern times have always lapsed into atheism ; and therefore the Theist must see that the matter does not end in a controversy as to the principles of morals, but ends in a controversy as to whether there is, or is not, a God. For, if we admit there is a God, I think we must admit that man is a responsible being, as man's responsibility is entirely derived from the existence of God. I think there is one thing that the whole of this system of Utilitarianism sets before us in a very full manner, and that is, the utter incapability of such a system as this, to account for the history of the world, or what we know to be in existence amongst men. It is a system which must essentially ignore moral evil and sin. I cannot conceive how moral evil and sin can have any existence under such a system as this. If, however, we were to use this vague term of Utilitarianism in another way, and ask ourselves what we know from the history of the human race to have been the most useful system of morals, I think that we might well test Utilitarianism, as contrasted, I won't say with Christianity, but with Stoicism, or any of the Theistical principles of the heathen world. What has most conduced to human happiness in this world ? Has it not been a principle which has always been Theistic in its origin, a system of morals admitting the existence of evil in the world, and seeking the aid of the Creator to diminish the evil that exists in the world ? The "greatest happiness principle," no doubt, can only be found in Christianity. (Hear, hear.) We may test and try all other systems by the experience of the human race. What is there in Christianity which causes those who are in the midst of bitter trial and suffering—who seek here for no happiness—who only know here pain and suffering—whose whole religion is consecrated to the following of One who was made perfect in suffering—I wish to ask, How is it that that principle conduces to the greatest amount of happiness even

here in the midst of suffering ; that it contributes to that feeling, when every other system of man's invention has been found to be utterly and entirely faulty ? (Hear, hear.) How, upon such a principle of morals as Utilitarianism, could you go among the heathen or the neglected outcasts of our own population, and bring happiness home to them ? Where, upon such a principle as this, could you find the men who have devoted their lives, not doing that which they conceived conduced to their own happiness, but for the benefit and the good of their fellow-Christians, altogether irrespective of any Utilitarian doctrine of happiness for themselves ? (Hear, hear.) It is true that Christianity shows that all that it inculcates will ultimately conduce to happiness, but it teaches man that that happiness is only to be arrived at through suffering. I think we should test Utilitarianism—for I think the fairest test of any system of morals is to bring it in contact with the history of the human race—by asking what it has effected for the human race ? and why it is to be substituted for Christianity,—why Christianity is now to be taken away from men, as a thing unsuited for the philosophical age in which we live, and another system made a substitute for it, which is to be essentially atheistical in its character, and, according to its own showing, is only by a species of slow development to lead man up to anything like that amount of happiness which Christianity has already afforded him ?

MR. REDDIE.—I am almost sorry that the unanimity prevailing this evening has left me little to do except to thank you for the very kind—I am sure much too kind—manner in which you have received my paper. I should have been glad if another paper had been read this evening, as you know ; and I should really have been better pleased now, if this paper had been criticised. We miss some of our usual members this evening, or, perhaps, it might have had the benefit of some adverse criticism ; for I feel sure there must be some weak points in the paper which it would have been desirable to have had pointed out to the author. There is, however, one consolation I feel, and that is derived from having elicited the remarks of Dr. Thornton and yourself upon this subject. I would make one remark with reference to the observation of Mr. Browne. I do not think that Dr. Thornton's use of the text of Scripture he employed has been quite as clearly advocated as I should like it to be. Dr. Thornton argued that you could only arrive at a proper appreciation of the beauty of goodness by the cultivation of the moral taste ; and in saying this he was arguing in a perfectly philosophical manner and from human experience ; but he also took the words of Scripture to show that there was in Scripture an anticipation of our philosophy as to this, an appreciation and enunciation of that same principle, not put forward philosophically—because nothing is put forward “philosophically” in the Scripture—but yet a previous knowledge and recognition of that very principle which we arrive at only by slow degrees—and that it is to be found in those texts the language of which he made use of to express his own philosophical conclusion. I think the argument of Captain Fishbourne was also a very pertinent one. Mr. Mill tells us that, in order to complete the theory of Utilitarianism, many Stoical (or, I think we

may fairly say, Theistical) and Christian principles are necessary. That being the case, I think there would be no departure from a truly philosophical mode of argument, even were we to make direct use of passages of Scripture in discussing Utilitarianism. I myself in my paper have been obliged to make use of them, because I am forced to show that the principle enunciated by Mr. Mill, as a new discovery and as deduced from his theory of Utilitarianism, is positively a plagiarism from Christianity. The duty of men to be at unity with one another is a principle of Christianity. But it was not merely taught by Christ, but even in the Old Testament; for we, Christians, do not acknowledge there have been two true religions. The religion of the New Testament is merely the religion of the Old Testament more fully taught, and made plainer and patent to the whole world, instead of being confined to a chosen people. And when we find Mr. Mill telling us, without going to the origin of that doctrine, that if we would *now* teach the principle of unity, we should have something which would re-convert the world,—we are surely entitled to point to the fact, that this teaching is not new, but that it is old; and that, if it has failed, it is from no defect of the principle, but because people, knowing what is good, will yet do what is not good. This is an unfortunate truth, with which we know the heathen were acquainted, from the well-known poet's reflection, "*Video meliora proboque, deteriora sequor*;" and the Apostle St. Paul also taught the same thing, with reference to his personal experience before he was converted to Christianity and became a consistent follower of Christ.

The Meeting was then adjourned.

ORDINARY MEETING, MAY 6TH, 1867.

THE REV. WALTER MITCHELL, VICE-PRESIDENT, IN THE CHAIR.

The minutes of the previous Meeting having been read and confirmed, the Honorary Secretary announced that, in return for our *Journal of Transactions*, the Royal Institution of Great Britain had, through its Secretary, presented the Institute with a complete set of its Proceedings from 1851 to 1866, in four volumes, and that three pamphlets had also been received from Mr. Patrick McFarlane, a Member of the Institute.

The following Paper was then read :—

ON THE LOGIC OF SCEPTICISM. *By the* REV. ROBINSON THORNTON, D.D., *Head Master of Epsom College, Mem. Vict. Inst.*

THE conclusions, or supposed conclusions, arrived at by modern science in opposition to the statements made in the Books which we accept as containing a Divine Revelation, have been generally parried by throwing doubt upon the facts or observations on which they are founded. Believers in the genuineness and authenticity of the Old and New Testament have been contented to cast discredit upon the accuracy of observers, or have even been tempted to accuse them of misrepresenting or inventing facts, for the sole purpose of subverting the authority of the writings which others held sacred. This accusation may possibly be merited in some few cases. Hasty observations may have been reported as nice and careful: inferences may have been registered as facts: and without doubt observations have received a direction, and reports a colour, from a foregone conclusion. But it would be doing a great injustice to the majority of those who advocate the views which our Institute was founded to combat, if we attributed to them any design but that of arriving at truth by means of truth. We contend that observations have been incorrect, and facts mis-stated, not that they have been deliberately falsified. However, it is not sufficient to impugn the records of the senses. Cogent argument as it is, if we are able to

point out a case where the observer's sight or hearing has deceived him, or where a statement, by passing from one to another, has been converted into something like the contradictory of its former self, it is rarely we can produce it. Throw what doubt you will upon the accounts of things seen, heard, analysed, discovered, you cannot expect to find modern science at fault in that which is perhaps the chief among her many glories, a rigorous and careful system of observation.

But while it is unfair and one-sided to impute evil motives, or even to suggest failure on the part of a practised observer, and somewhat suicidal to weaken the value of facts which may after all tell on our side, there can be no objection to our sifting diligently the logic of sceptical arguments, and showing that whatever the state of the case may be as regards the correctness or incorrectness of the facts laid down to argue from, the mental process is not free from error. I must not be considered capable of the presumption of attempting to execute such a task for the whole, or even a part, of what is alleged against Scripture; and indeed it is scarcely our province to thrust ourselves into controversy: my object will be to call attention to the nature of logical processes in general, and so to point out where it is that we may expect to find the weakness of the weapon aimed against the believer in the absolute truth of our written Revelation.

Logic is defined as "The Science of the Laws of Thought." Whether this definition be adequate or not, we will not stop to inquire; but will go on to define a logical process as "the passage of the mind from one thought to another." By "thought" I here mean, not a simple notion, but a compound notion, asserting something concerning the relation of two or more simple notions. This passage or movement of the mind is, like all other motions, subject to its own laws; but there is this difference between the motion of intellect and of matter, that while the latter cannot take place at all except according to law, the laws of mental movement may be apparently, but not really, obeyed; or, in other words, to get rid of the fallacy latent in the word "law," physical motion is variable only within limits; intellectual motion may vary infinitely, though one movement only conducts to Truth.

This movement of the intellect from one thought to another is itself called by the name of "Thought." The superior power of the Greek language enables it to distinguish (which we cannot do) between "a thought," *i. e.*, the object or fact we think of, considered with reference to our own mind (*νοούμενον, νόημα*), the act of thinking (*νόησις*), and the passage from one to another, "Thought" simply (*διάνοια*).

Those notions of the relation of simpler notions which I have called "thoughts" are obviously of different kinds. We may have a relation between class and class, or between individual and class, or between individual and individual. And so by a simple calculation we may see that there are nine different processes of the mind: from the relation between classes to another between classes, or between individual and class, or between individual and individual; or, again, from the relation between individual and class to that between classes, or from that between individuals to that between classes, or between individual and class; or from that between individual and class to another between individual and class, or between individuals, or from a relation between individuals to another between individuals. The first three of these processes coincide mainly (for I shall not weary you by analysing too closely) with what is termed Deduction, or Synthesis; the next three with Induction, or Analysis; the remaining three, though least scientific in appearance, are as a matter of fact the commonest processes of all. We habitually reason from individual cases to individual cases. It is the opinion of many logicians that in such reasoning we insensibly generalize and particularize again; they conceive that a process from individual to individual is impossible, and that the mental road lies through a universal. With all due deference to high authority, I am inclined to maintain the opposite, and to hold that the mind does actually proceed from one individual notion to another, without passing through any induction, rapid or slow.

Each of these mental processes has its own special law or rule of guidance. The law of Deduction is expressed—or was intended to be expressed—in the "*dictum de omni et nullo*" of old logicians. We may term it the law of "Universal Truth." Granted a general proposition, it is equally applicable to every case *which comes under it*. Granted a relation between classes, that relation holds good for every portion of those classes.

The rule of Induction may be characterized as "the law of Uniformity." Observed a fact with regard to an individual, *supposing that individual to be the adequate representative of a class*, you can infer a class-relation.

The law of the third process of the mind I shall term "the law of Analogy." Observed a fact with regard to an individual, you infer a similar fact about another *similar* individual.

If these somewhat broad statements about the laws of the three mental processes be taken as in the main correct, we can easily see where error may arise; namely, from some violation of the special law which regulates the truth of the process.

Deduction has been fairly enough termed (by Mr. Mill) the deciphering of our manuscript notes. As far as mere positive science is concerned, this description (for description it is) of Deduction is correct. It is when we come to Geometry, Psychology, and Theology, that we find the difficulty of acquiescing in the application of the name "manuscript notes" to the intimations of a Creator's Will and Being, and the necessities of thinking to which He has subjected our intellect. But let the term be accepted. It is plain that the "law of Universal Truth" requires, for a correct passage of thought, that the relation inferred should really be contained in short-hand in the manuscript notes: that the individual case to which the general is applied *really does* come under it.

The fallacy then will be either to introduce a false or unsuitable relation; or else to apply a suitable enough relation to a case which seems to come under it, but does not actually.

Of the first form of fallacy none is commoner, none more in use among sceptics, than that which is called the *argumentum ad verecundiam*. We are told, You must allow this, you must deny that; and when we ask why, we receive the reply, "Because Professor A. or Mr. B. has said so. If you do not acquiesce you are guilty of the presumption of doubting them." The argument is transferred from the truth of fact α or fact β to the credibility of A or B.

I should not have alluded to this form of fallacy were it not for the fact that the sceptical school resolutely deny to believers the argument from authority, while they themselves use it. If we urge the acceptance of Scripture because it has been accepted by so many, by thinkers and by workers of so many ages, and such varied modes of life, we are told immediately that the question is one, not of opinion, but of truth; that it shows a blind deference to the unreasoning credulity of ignorant ages to plead for the acceptance of a book because it has been accepted for two thousand years. On the other hand, if we venture to prefer our Scripture to the somewhat vague and uncertain generalizations of geologists, and the like, we are met forthwith with the authority of learned names and ordered "*favere linguis*."

I call this pushing the "argumentum ad verecundiam" too far, an instance of the fallacy of false or unsuitable relation, because, instead of having the relation between class and class (or between individual and class) clearly pointed out, we have merely given us the dictum of an individual concerning that relation.

But, supposing the relation clearly and adequately stated, we come to another form of fallacy; that of proceeding to

another relation apparently, but not actually, connected with the foregoing, and so not really a portion of the Universal Truth. To such fallacies as these Aristotle devotes a whole treatise; and there is scarcely a logical writer who has not touched upon them. I wish to call attention to two, which appear the most common.

The first is technically called "*a dicto secundum quid ad dictum simpliciter*." A statement having been made, with certain limitations and qualifications, these are tacitly put aside, and the statement employed as if it were made without them. Thus, when we allow the singular phenomenon of *parthenogenesis*, as an exceptional mode of propagation, to be accounted for by peculiar physical circumstances, we may be considered to have acquiesced in the possibility of its being the rule rather than the exception. Or, when we quite agree with the truth of Mr. Darwin's pigeon experiments, and allow that, *within limits*, varieties almost infinite in number may be produced almost at will, we may be taxed with granting that similar variations may take place, and be perpetuated, out of those limits.

The technical term for the second of these fallacies is "*ignoratio elenchi*." The word *elenchus* signifies here the contradictory of the proposition which is opposed; and the fallacy consists in "ignoring the *elenchus*,"—that is, substituting for it, and proving, instead of it, a proposition something like it, but not incompatible with the proposition in question.

As an instance of the *ignoratio elenchi*, I may bring forward the manner in which the miracle of the battle of Beth-horon is dealt with. The Scriptural language on the subject is, as we might expect, popular, and not scientific, and has moreover a poetical cast. "The sun stood still." The opponents of Scripture meet this by showing that, as the sun does not move in the heavens, it need not be commanded to stand still; and even if we understand the words of the diurnal revolution of the earth, such an utter confusion of all things would occur from its suspension that we cannot conceive a Deity of law and order sanctioning such an invasion of His system. Without going into the theological question of the nature of the Divine power and will, I think we may call this an *ignoratio elenchi*. What Scripture in effect states is, that for some reason or other, not given, the sun's light was visible, and the sun himself appeared in one place, longer than usual. The "*elenchus*" of this would be, "The event did not happen at all;" or, "It is hardly conceivable that it should happen in any way consistently with what we know of the Divine order."

The proposition actually proved is, that the event did not happen in a particular way—viz., by arrest of motion: a proposition by no means incompatible with the perfect truth of the narrative of Joshua.

These three fallacies appear to me to be those which are most commonly to be found in the Deductive logic of Scepticism. That other violations of the law of Universal Truth, as I have called it, occur in sceptical writings and arguments, is highly probable, if not morally certain; you will observe that all such false reasoning derives its falsity from the regarding as the portion of a class placed in a certain relation some class or individual apparently but not actually belonging to that class.

I come now to the fallacy of Induction: the neglect of the "law of uniformity." The individual case from which the induction starts must be, according to this rule, the adequate representative of a class; otherwise there can be no uniformity whatever. A false induction, therefore, is made where a relation between class and class is inferred from the relations of an individual not really representing, but only seeming to represent, one of those classes. There is no branch of science, I suppose, in which errors of this kind have been more rife, than geology. A number of facts having been carefully and patiently accumulated, geologists proceeded to their induction, and arrived, as they thought, at irrefragable universals, incompatible with the truth of the Scripture narrative. But their store of facts was not exhaustive. Some new and unexpected discovery has completely modified a proposition once regarded as almost axiomatic. I need only refer to the effect of the *Eozoon Canadense* on the appropriateness of geologic nomenclature; and the declaration of one of its most eminent professors, that the whole science must be remodelled.

This fallacy was the one against which the old Induction, "*per simplicem enumerationem, ubi non reperitur instantia contradictoria*," failed to guard. It is not the multiplying of affirmatives, and the absence of negatives, that constitutes a valid induction: it must be made clear also that if any negatives existed, they would be present; that the *instantia contradictoria* would be sure to be forthcoming if there were one. And thus we find that, to attain truth, we must (as Bacon saw) either be able to interrogate nature by arranging circumstances for ourselves, and so making an *experimentum crucis*—a hand-post experiment—or resort to some method of inquiry which shall eliminate all that is unimportant, and show us what is the real representative of the class whose relations we may be desirous to investigate. Logicians reduce these

methods to four—viz., the method of Agreement, of Difference, of Residues, and of Concomitant Variations. I mention these, not because they have any special reference to the logic of scepticism, but because, as I am on the subject of incorrect reasoning, I wish to point out the especial danger of error in the third method, that of residues. The rule for this method is thus given by Mr. Mill. “Subduct from any phenomenon such part as is known by previous inductions to be the effect of certain antecedents, and the residue of the phenomenon is the effect of the remaining antecedents.” And the same logician cautions the observer against possible error. “We must be certain,” he says, “that the residual antecedent is the only one to which the residual phenomenon can be referred: the only agent of which we had not already calculated and subducted the effect.” We must also be certain, it might be added, that the residual antecedent does not consist of many separate antecedents, one of which, and one only, is the real antecedent of the phenomenon, the rest being without effect. For if we are not certain of this we may attribute to certain inert circumstances a share in producing a phenomenon with which they had nothing to do. Thus we may fall into the error of attributing undue influence to conditions which really exerted no influence whatever, or may even select as the cause of a phenomenon that which has really no connexion with it at all. I might instance as an approximate example of this kind of error, the case of the Neanderthal skull. Its fossilized character, the absence of gelatine and chondrine, its position, and such portions of the phenomenon, having been accounted for, there remains its peculiar form. What is the reason of this? Subduct all other peculiarities as explainable and explained, how do you account for it? The sole antecedent which appears to remain is its antiquity; and if, in accordance with the method of residues, we attribute its peculiar shape to its age, we are led to the inference that the primæval race in that part of the world must have been a different race from any now to be found—*pithecoïd* men, if not anthropoid apes. But there is another possible cause which does not appear in the residue of antecedent circumstances, which I believe is now accepted as the true reason of the peculiarity of this skull: it is an individual distortion—an abnormal growth exhibiting itself among men, who were by no means *pithecoïd*, but such a race as the scriptural ethnology might lead us to expect to find settled in early times in that part of the world. And thus the sceptical argument against the truthfulness of the scriptural anthropology drawn from the appearance of this phenomenon, loses its support.

Closely connected with the fallacy of imperfect induction, or rather a form of it, is what I may term the fallacy of *negatives*. It is the case where the non-appearance of the *instantia contradictoria* is taken as equivalent to its non-existence: whereas, as was observed above, we must be sure that if there were any instances to the contrary, we should have heard of them or discovered them. This error is obviously sufficient to vitiate the whole of an induction. It is inductive in its character; but there is a fallacy which (*mutatis mutandis*) penetrates into the region of deduction, and which I should call by the same name, fallacy of negatives. It consists in taking that which is *not proved* as *disproved*. The overthrowing of one out of several arguments in favour of a certain conclusion does not prove that conclusion to be false; it only destroys one syllogism in its favour. It may be a very good axiom for practical purposes that *de non apparentibus et de non existentibus eadem est ratio*; but it will never do to lay down that everything not proved is false. As an instance of the fallacy of negatives, I may allude to the sceptical argument against the fact of the Resurrection, that it is not mentioned in Roman records. So accurately were they kept, it is said, that such an event must have been recorded, and discussed at Rome, either as a philosophically interesting fact, or as a religious portent requiring expiation. Now without referring to the explanation furnished by Scripture itself, viz., that the soldiers stated that they had slept at their post, and allowed the body to be stolen, and that the matter was hushed up by means of a large bribe, we may rank this argument under the fallacy of negatives. How do we know that the matter was not recorded and discussed as alleged? True, we do not find these records, we have no account of these discussions; but are we to infer that there were none? Is it not probable that hundreds of other remarkable events were duly reported and made the subject of conversation, of which no record remains at this day? But further, it has always struck me that the argument against a total deluge, drawn from the state of the extinct volcanoes in Auvergne, exhibits this fallacy. I must not be understood to be expressing any opinion,—though I have one,—on the subject of the total or partial nature of the Flood; I am only discussing the logic of a particular argument. As I understand the reasoning, it is this: the appearance of certain volcanic craters in Auvergne is such as to show indubitably that they have not been covered with water since their last eruption. Now there is no record of any eruption having taken place there within the memory of the human race, nor any tradition of their ever having been active. Consequently the last eruption

must have taken place before the creation of the present race of men, and therefore before the Flood; and therefore the Deluge must have been partial, or else it would have altered their appearance. The whole argument falls into a syllogistic form thus:—If there has been no eruption since the Flood, the mountains would have exhibited traces of the Flood, supposing it total; but there has been no eruption since the Flood; therefore the mountains would exhibit traces of the Flood, if total. If the Flood were total, the mountains would exhibit traces of it; but they show no such traces; therefore the Flood was not total. This is what logicians call technically a double hypothetical, first constructive, then destructive. No possible doubt can exist of the truth of either major, considered as a hypothesis; and the minor of the second hypothetical is a matter of observation. The correctness of the argument therefore depends on the correctness of the first minor, “no eruption has taken place since the time of the Flood.” This proposition is proved as follows. No event of which there is no record ever took place; a post-diluvial eruption of these mountains is an event of which there is no record; therefore none such ever took place. This syllogism is correct in form; but the major is palpably false, and I rather think the minor is not altogether certain. I believe that allusions have been found to a volcanic eruption in or near the district in question; and we know from the example of Vesuvius previous to the eruption of 79 A.D. that a long period of inactivity is not impossible in a volcanic district. The fallacy of negation is contained, however, in the major. Is it true that no unrecorded event ever took place? Are we to suppose that the rude Kelts, or the still earlier Fins, or Euskara, of the country we now call France, preserved any records or traditions of natural phenomena? Are we to suppose that the Roman invaders, in 125 B.C., would have cared to collect and retain such records and traditions, had they been preserved up to the invasion? or that the Greek colonists of Massilia in 600 B.C. would have carefully handed down to their children the vague traditions of a number of savages? Nay, more, have we in our possession all the papers and documents treating of the physical aspect of Gallia Braccata, so as to be certain that none of them mention a tradition of the Arverni, that Divine fire had once been kindled on the summit of their hills? Men must be prepared to assert the probability, at least, of all this, if they employ this argument in the manner I have described. If they are not prepared to make such an assertion, their argument is fallacious.

While I am on the subject of errors connected with inductive reasoning, I must not omit another fallacy, which can be

traced in several sceptical arguments. I may call it the fallacy of *supposed* uniformity. The law on which all analytical argument depends I have termed the law of uniformity; but before we press such an argument, we must be sure that the uniformity really exists. If we are not certain of this, we shall be liable to fall into the error of making cause disproportionate to effect, or effect to cause. The most remarkable instance of this fallacy is to be found in the arguments alleged against Scripture history, drawn from the thickness of deposits on the banks of the Nile and elsewhere, and from the finding of pieces of pottery at a depth in alluvial soil, which seem to show that human art existed many centuries before the period which the Bible seems to assign to the creation of man. All these arguments depend on the supposition that deltas and other alluvial deposits are uniform in their growth; that a river bringing down silt with it deposits exactly the same thickness yearly now that it deposited thirty-five centuries ago; and that we may accordingly calculate unerringly, from the depth of a deposit, and the present rate of its deposition, how many years, or millions of years, have elapsed since the first layer assumed its place. But is this true? I apprehend that those who have been at the mouth of the San Juan, or the Aspro-potamo, or in the Chinese seas, will be of a different opinion. The rate of deposition is not necessarily uniform; and the potsherds found deepest in the Nile mud need not be earlier than the time of the first settlement on its banks under Men the Hamite.

I come now to the third logical process, that from one individual relation to another, guided by the law which I call the law of analogy. What the fallacy is to which this process is liable, we may easily see. The guiding law is violated by passing in thought from one individual to another which is not really but only apparently similar; by contenting ourselves with a hypothetical likeness, and so employing a false, not a real, analogy. The tendency in our minds, which I have already pointed out, to prefer the process from individual to individual, renders this fallacy of false analogy one of the commonest and yet often least easy to detect. The most effectual mode of exposing it seems to be that of completing the whole intellectual route, and, instead of passing directly from individual to individual, supplying the law of general probability under which both come. If I throw up a stone and find that it turns and falls down, I infer that if I throw up another it will do the same. The mind passes from individual to individual by the law of analogy. This law is really subordinate to that of uniformity, and each analogy, to be correct, ought

virtually to contain within itself an induction and a deduction, arriving at, and returning from, a general probability. Thus (in the instance just given) the correctness of the analogy is shown by reasoning thus:—This stone falls; what is true of this stone is probably true of all stones, this one being, as far as I can see, an adequate representative of the class: if, then, all stones will (probably) fall when thrown up, this other stone will (probably) do so. The probability is inserted as a modification, because there is no opportunity of testing accurately whether the stone in question is an accurate representative or not of the whole class. If there were such an opportunity, a genuine induction would be the result; where no testing can take place, we must be content with probability. This rapid seizing of an analogy, and either working from one to other individual cases, or summing all up in one grand induction, was not unknown to the Greek logicians, and seems to be what is spoken of by Aristotle under the name *ἀγχίνοια*, or nearness of reason. But *ἀγχίνοια* has its dangers. The similarity between individuals, which it lays hold of, may be in accidentals, and not in essentials, and the real essential differences may lie exactly where they are last to meet the view.

The sceptical reasonings relative to the criticism of Scripture appear to contain this fallacy. Scripture, it is urged, is a written document, and should be subjected to the same process of examination as every other writing. Scripture history is a collection of legends, and must be interpreted just like all other historical legends. If Romulus is a myth, so is Moses; if the supernatural appears in the battle of Rephidim and the battle at the Lake Regillus, it is as incredible in the one tale as in the other. Here we have an instance of false analogy. Scriptural documents are not like others, because they put forth different claims on our belief. Scriptural tales—legends, if you please to call them so,—do not stand on the same footing as heathen traditions: they were committed to writing, by the confession of their opponents, at a period far earlier than that at which any other human records were written, a few undeciphered hieroglyphics, perhaps, excepted; they have continued to be accepted and believed by a large number of persons, and have been quoted as authentic history, even to the present time, while other legends have long been relegated to their true place, and though, perhaps, not scientifically interpreted, have still not been put forward as giving the literal truth. Hence the analogy drawn is false: Scriptural stories are not representatives of the class to which historical legends belong, nor are the myths of heathenism fairly

specimens of the class of documents which claim a divine origin, and have long been allowed their claim.

I am sensible that I have not gone so deeply as I should have gone into psychological inquiry with respect to the direct process of the mind from individuals to individuals. The whole subject of the analogy of individual relations is one of deep interest, and especially to the theologian, to whom a single soul, with its special powers, trials, dangers, and aids, is an object for reverential study. Perhaps some member of our Institute, whose leisure for thought and powers of thinking enable him to work the subject more thoroughly, will take up what I have thus somewhat presumptuously ventured to touch on. We need a "Kritik" of the whole process of reasoning by analogy. There is another "Kritik," also, which logical science appears to need—a criticism of, and canons for, the Logic of Contradictions. For we must remember that every sceptical argument aimed against Scripture involves a double process: the establishment, or, at least, the assertion, of a certain proposition, and the comparison of this proposition with the propositions enunciated in Scripture on the same subject. Here we have three possible fields of error: the logic of the sceptic, the interpretation of Scripture, and the comparison of the two propositions. I have already endeavoured to point out where sceptical logic, constructively considered, may be possibly found to fail, and we leave to Exegetical Theology to determine what Scripture really does assert. Doubtless the Bible has often been made to say anything but what it does really say, but the investigation of its import belongs not to Philosophy. However, suppose the statement of Scripture to be clear, and the scientific conclusion alleged contrariant thereto to be logically correct, we have still the comparison between the two to examine. May it not often happen that two propositions, apparently contrary to one another, are really, in logical language, only sub-contrary, capable of being true together; representing, perhaps, two different sides of the same ontological truth,—two equally necessary canons,—but referring to different conditions of being? We know it to be true that all men are mortal, and still, in spite of logic, just as true that no men are mortal. The ambiguity in the word mortal is easily detected here: may not a deep thinker's rigorous "Kritik" of the whole subject of contradiction clear away many a supposed discrepancy between the Book of Nature and the Book of Grace?

I must conclude this paper, as I did one which I had the honour of reading before this Institute about a year ago, with

an apology for having made so few references, and cited no authorities for my statements. I have designedly abstained from so doing, for I am alive to what has been well called the fallacy of quotations. A visit to a library, or a reference to one's own bookshelves, would enable one to swell a paper out with long passages, relevant or irrelevant, from Pacius and Zabarella, from Petrus Hispanus and Salabert, from Hamilton and Mill. But I repeat what I said then, that our object is not to show what men have thought, but to induce others to think. The only weapon which mind can use against mind is mind itself: σοφία γὰρ ἂν σοφίαν παραμείψειεν ἄνθρωπος.

The CHAIRMAN.—I need not ask you to return thanks to Dr. Thornton for his interesting and very learned paper. I am sure it is one we shall all value very much, and one which will require deep study. It is almost impossible to take it in fully from merely hearing it read, but if any gentleman has any observations to make we shall be glad to hear them.

Mr. INCE.—Dr. Thornton does not seem to be aware that between the years 400 and 500 A.D. those mountains in Auvergne were in active volcanic operation, and that there are records of the fact in existence still, in letters from the Bishops of that part of France to other Bishops, begging their prayers during the prevalence of that calamity. I have shown that document, which I extracted from the *Quarterly Review*, to Mr. Reddie, and I will take an opportunity of showing it to Dr. Thornton.

Rev. ROBINSON THORNTON.—I have alluded to this, though, it seems, not definitely enough, in my paper; and the reason I did allude to it, was because I had had the pleasure of hearing Mr. Ince make that important statement once before. I was then interested in it, and it was in my mind when I put in the paragraph, "allusions have been found to a volcanic eruption in the district." But I did not like to say more, because I did not wish to "take a plum out of his pudding."

Mr. REDDIE.—I think it would be interesting to our members to have this circumstance which Mr. Ince has alluded to, and which I supposed Dr. Thornton to have had in view in that passage of his paper, extracted from the article in the *Review* in which it appears. I am sorry that other occupations prevented me from getting hold of the passage and citing it this evening, but I shall endeavour to append it as a foot-note in our *Journal of Transactions*, our object being to make all our discussions as full and complete as possible.* One question I should like to ask Dr. Thornton, with reference to an old friend of mine—the Neanderthal skull. There is a passage in his paper that I do not quite understand; he says that this was probably a skull of "such a race as the Scriptural ethnology might lead us to expect to find settled in early times in that part of the world." Although he very properly calls it "an individual distortion,"

* *Vide* NOTE, p. 166.

"an abnormal growth," he still appears to expect we might find a race of people all possessing this abnormality and distortion, although he also described it as "individual." I do not quite understand what view Dr. Thornton intends to express on the subject; but as far as I understand the state of the case with reference to the Neanderthal skull, it is simply this: that it is a purely individual and exceptional distortion arising from a disease known as synostosis or ossification of the sutures. The human skull, as you know, is divided into parts which fit into one another, so as to allow room, however, for the growth of the skull, as the child grows into the adult and afterwards into manhood. In the Neanderthal skull these saw-like divisions have become ossified and stuck together; and there not being the ordinary means for the skull enlarging itself normally in every direction according to the growth of the brain, the skull has grown in a distorted form, and more particularly towards the forehead, by the pressing out of the frontal sinus, thus giving a depressed form to the head. There can be no doubt about this fact. Dr. Barnard Davis made a careful examination of the cast of the skull; and I have never heard it questioned by a single individual, since he put out his valuable memoir on the subject, that that was the state of the case. The skull has, therefore, nothing of a race characteristic. Of course, it is perfectly possible that the heads of people living in a certain state of nature, without very much study or anything to occupy them of an intellectual kind, and with all their faculties of observation constantly exercised, arising from their being engaged in war, in hunting, and so on, might, if there is any truth in phrenology, naturally tend to develop strongly over the ridge of the nose, and this might also prevent the elevation of the head, where the organs of veneration and benevolence are supposed to be situated. An instance of the reverse kind, in a people highly civilised, though their civilization is different from ours, may perhaps be found in the Japanese. I think any one who has paid a visit to that very interesting exhibition by the Japanese Jugglers, now in town, must have remarked, that, from the youngest child there to the oldest person amongst them, their heads are peculiarly developed where the faculties of reflection predominate, their foreheads being extremely elevated, and the children's remarkably so. Besides the Neanderthal skull, Dr. Davis has, I believe, the casts of some British skulls, the history of which is known, and which are developed in the same abnormal way as it is, from the same disease, the sutures being ossified. All this can be explained in a natural way as an abnormal development; and does not imply anything like a race characteristic. I do not understand why we should suppose that it does.—

The CHAIRMAN.—Dr. Thornton has guarded against the idea of a race; he says, "an abnormal growth exhibiting itself among men, by no means *pithecoïd*—but such as the Scriptural ethnology might lead us to expect to find settled in early times in that part of the world."

Mr. REDDIE.—But I do not understand why you should expect from the scriptural ethnology, that there should be a race of people all having synostosis in parts of the world settled in early times, if that is meant. As to this I should like some explanation; and I only add that I have heard this

question discussed by all parties—both by those who wish to make out the skull to be *pithecoïd* and the reverse,—and I have never heard Dr. Barnard Davis's conclusions once questioned. We have some gentlemen now present, capable of giving an opinion, if they will be kind enough to do so ; but I do not think there is anything in the Neanderthal skull to lead us to expect that there ever was a race of people settled in the world who had skulls anything like it.

Dr. THORNTON.—I never imagined the skull to be the representative of a race, but an individual distortion. The race settled in that part of the world in early times, I conceive to have been Fin ; but it is not necessary to enter on that subject now.

Mr. REDDIE.—I am glad to have elicited this explanation, which I see is quite consistent with what the paper says. I believe one of Dr. Davis's abnormal skulls is that of an Irishman—a Celt ; and I suppose no one race is more subject to synostosis than another.

Mr. WARINGTON.—I confess I am somewhat sorry for the title of the paper, though as regards its matter I should agree with it very well. The impression which that title is likely to convey, and which I suppose it was meant to convey, is that there is a peculiar lack of logic in sceptical objections. I am quite aware there is a lack of logic ; the only thing I question is its peculiarity. When we look around and observe the way in which men of science, or indeed men generally, are in the habit of drawing conclusions, we see, that in cases where theological prejudice has not the slightest influence, they are so perpetually falling into the very same logical errors, that it is plainly unjust to them to suppose that when they do so in opposition to Scripture, it arises from any peculiarity of the position in which they are placed, or of the object which they have in view. I am quite aware that in the substance of the paper Dr. Thornton has not expressed himself at all strongly in this way. But it strikes me that in speaking, not of the fallacies of scientific origin, but of the logic of *scepticism*, the impression is given that these fallacies are in some way characteristic of sceptical objections, and are not to be found elsewhere. To remove that impression I would briefly point out a few cases in which there are similar errors observable on the other side. There is another kind of scepticism as injurious at times, or even more so, than that of which we have heard to night : viz., theological scepticism in regard to science ; a scepticism which has certainly done a good deal to cause the breach at present existing between Scripture and Science. On purely theological grounds, men have been sceptical of science, and in being so have fallen into the same fallacies of argument as men of science on the other side. I will not go through all the paper, but I will take one or two instances by way of example. First, as to the argument from authority, that A. B. says such and such a thing is true, and therefore it is true. Well, Dr. Thornton has himself hinted that the thing is done over and over again by theologians also, who, when an assertion on the side of Scripture is questioned, do not trouble to go themselves and find out whether this statement is really a statement of Scripture or not, but say, “ Oh ! Dr. A. B. says so ; do you object to

his authority?" It is exactly the same fallacy, and I confess I do not see why the dictum of an extremely learned scientific man is to be less received than the dictum of a learned scholar. In both cases their knowledge is imperfect, they make mistakes, however learned they may be, and so there would seem to be about as much worth in the one as in the other. Again, there is the fallacy in regard to the particular interpretation which we choose to put upon phenomena, and which we regard as, in consequence, a part of the phenomena, when it is really only an inference of ours. Take an instance from Dr. Thornton's paper, about the sun standing still. It may not have entered into the minds of those present to question whether Scripture really teaches that the sun did stand still, yet it is an extremely doubtful point. The original of the word is "be silent." The sun "was silent" in heaven. It is shrewdly supposed by some (and I can find no objection to such an interpretation) that it refers, not to light, but to prolonged darkness; that there was a great storm at the time, during which stones fell from heaven, and, as an attack in the dark is usually more fatal than an attack by day, Joshua prayed that the sun might remain as it was, dark and silent; that it did so remain for the whole day, there was no light, but the battle went on in the dark, so that there was no day like that, before or after. I do not say that this is the true interpretation, but merely adduce it as an instance of the way in which what we have been accustomed to hear as the teaching of Scripture may prejudice us, and make us regard what is really a mere inference as part of the fundamental facts. Then again, there is the fallacy which Dr. Thornton notices, in the imperfect subtraction of known causes, and the effects they will produce, and the consequently fallacious reference of the remaining facts to some other antecedent. This is also constantly done by the opponents of sceptics. They say, for example, that men of science have failed to account for the deluge on scientific principles, that they are unable to show natural causes sufficient to occasion it; whence the conclusion has been jumped to—"Then the deluge was miraculous." Wait a moment. Are you certain that every cause is known which could account for it, or that, of every cause with which you are acquainted you know all the effects? I think not; but if not, then the reasoning is plainly fallacious. There are a considerable number of cases of this kind, where men jump to the conclusion that a thing is proved to be miraculous simply because not disproved to be so; in all which cases there is a liability to this kind of fallacy. I take a few instances thus (one might go through nearly all the points of Dr. Thornton's paper in this way, and parallel them with other examples), not for the purpose of dwelling upon the logical errors of defenders of Scripture, but merely to remove the impression that sceptics are more illogical than others. I believe theological scepticism has extremely little effect on the process of scientific reasoning. I can imagine a man with sceptical opinions, using a half-established conclusion, apparently antagonistic to religion, as if it were one fully proved, and this, I apprehend, is the true account of most such inconsequential reasoning; but you can hardly call this a logical fallacy, for it is not a deep-lying sophistry, but appears plainly on

the surface. With that exception, I do not think scepticism has much to do with making men of science reason illogically, and I must say, when there are so many glass windows in the houses of those opposed to scientific scepticism, that it is not wise for them to throw too many stones.

Rev. CHARLES DEANE, D.C.L.—I do not rise to meet the observations of the last speaker, although I think they are subject to question ; and I should differ from him almost entirely as far as the writers of the present day are concerned. I think they are going away from authority as a rule, and seeking proofs from Scripture, rather than accepting the dicta of the divines preceding them. But I do not rise to combat that proposition of Mr. Warington, but to request you, Sir, to ask him to tell us if he can, what is the original of the remainder of that verse which he referred to, with regard to the sun standing still, or “being silent.” Our version says, “And hastened not to go down during the day.” If Mr. Warington can remember the context, I think it would help us in considering the point, whether the sun was merely “silent,” or if we must believe that the sun really stood still.

Mr. WARINGTON.—The only alteration that would have to be made, to make that verse correct, is to strike out the word “down.” The expression may be used either of the rising or the setting of the sun ; it simply implies motion ; and the expression “The sun was silent and hastened not to move,” would plainly suit either interpretation which might be put upon the verse equally well.

Rev. J. MANNERS.—I wish merely to refer to Mr. Warington’s interpretation of the original passage. I believe “be silent” is a literal translation of the Hebrew. Now, you could have a darkness that might prevail for any length of time, whether the sun moved or not ; and there might be darkness in one place and light in another.

The CHAIRMAN.—Dr. Thornton mentioned this as showing an instance of want of logic on the part of some sceptics. “The Scriptural language on the subject is, as we might expect, popular, and not scientific, and has moreover a poetical cast.” I think Mr. Manners will find he is at one with Dr. Thornton, while he does not differ from Mr. Warington. He only mentioned an additional fallacy to the one mentioned by Dr. Thornton—a fallacy of some who support the miraculous view, without going themselves to the Scriptures to determine what the Scriptures really said on the subject. I am sure it would be great presumption on my part to discuss so learned a paper as Dr. Thornton’s, without more time to prepare for it. I can only say, that I do think,—and I differ from Mr. Warington in this,—that “The Logic of Scepticism” is a very proper title to the paper. But Dr. Thornton has by no means maintained the counter proposition, that there is nothing illogical on the part of the defenders of revelation ; and surely it is valuable for thinking men to have especially set before them, what is illogical in those objections which are urged by sceptics against the Scriptures. Dr. Thornton gave a very valuable classification of these fallacies ; and I think it is very important that our members, those who are not logicians themselves, should be aware of them. Many people of tender faith may find their faith confirmed, when

they are shown that that which is apparently illogical can be defended, after all, on more strictly logical grounds than the objections themselves. If I may make any further observation it is this, that the whole of the paper appears to me to prove,—and I think it will so convince those who read it,—how difficult it is to argue logically upon any subject whatever. Nothing is more difficult than a strictly logical argument; and therefore when sceptics come forward with what may appear to be a strong logical argument, it ought to be the office of the defenders of revelation, in the first place, to examine very minutely and strictly the logic of the sceptic. If I wished to adduce an instance to show how difficult it is, even for a profound logician, to argue and reason logically upon a subject with which he is not extremely familiar, I should not have to go far for an instance. I will not take a matter with regard to revelation, but a scientific matter; and I shall go no further than to the treatise of Mr. Mill on Logic. Very early in this, he gives as an exemplification of a strictly logical process the demonstration of the 5th proposition of Euclid, incorporating into the 5th the 4th proposition. Now, through every edition of Mr. Mill's Logic, a fallacy has been allowed to slip into this famous *pons asinorum*. I would say with all deference to the logical powers of Mr. Mill, that he has failed in passing the "Asses' Bridge!"—not because he is a bad logician, but because he was writing upon a subject with which he is not extremely familiar. If he had been extremely familiar with the methods of reasoning in Euclid, he could not have fallen into the fallacy he has. But any person who will carefully examine the mathematical demonstration of the 5th proposition of Euclid, incorporating the 4th, will find he has committed there a grievous mathematical blunder and fallacy, and I think this is a thing to caution men. Not only must a man be skilful in logical processes, but he must apply those processes to a subject with which he is familiar. That want of familiarity with a subject, though a man may be well armed with all logical processes, will cause him frequently to make a slip. I shall now call upon Dr. Thornton to reply.

Rev. ROBINSON THORNTON.—It is scarcely fair to call it a reply, for I think all that has been said has been much in my favour, and has tended to bring out matters which I was unfortunate enough to leave neglected. The only remarks on which I have to make further comments will be those of Mr. Warrington. In the first place, his criticism of the title of the paper is more lenient than I should have expected; for I am more dissatisfied with it than he is; and the only reason I adopted it was this—I could not think of a better; every other was worse, and I took this as a *pis-aller*. I agree thoroughly with him, that there is also a lack of logic on our side, but then I remind him of this,—the Victoria Institute was founded purposely in order to prevent the believers in Scripture having this constantly cast in *their* teeth. We are assembled and associated to examine science scientifically, and not theologically, and thus to meet the arguments drawn from science against the Bible; and the Institute is therefore a protest against that lack of logic. As regards the subject of Biblical Exegesis, I have not forgotten it; and I must remind him that I have expressly said, "We leave to Exegetical Theology to deter-

mine what Scripture really does assert. Doubtless the Bible has often been made to say anything but what it does really say ; but the investigation of its import belongs not to philosophy." I have said this in order to point out that I have not forgotten the matter. As to the battle of Beth-horon, my explanation was derived from no less a person than Dean Stanley, who takes the view that the day was prolonged. Not being profoundly versed in Hebrew, I am doubtful of the proper translation, but upon the whole I would adopt that of Dean Stanley.

Rev. J. MANNERS.—What does Dean Stanley mean by the day being prolonged ?

Dr. THORNTON.—That the light was allowed to remain visible for a longer period than usual.

The Meeting was then adjourned.

NOTE. (See p. 159.)

THE EXTINCT VOLCANOES OF AUVERGNE.

Mr. INCE has kindly forwarded to me copious extracts from the article in the *Quarterly Review* referred to by him ; and upon investigation I find that the matter is of still greater importance and involves more important issues, with reference to the whole question of "the Scriptures and Science," than would merely result from establishing as a fact that the volcanic cones of Auvergne had been erupted in the fifth century of the Christian era, and were not of the great antiquity ascribed to them in the first instance by Dr. Daubeny, and it would seem more recently by Sir Charles Lyell himself, not only in his *Antiquity of Man*, but in his *Elementary Geology*, the latter being cited by Dr. Colenso, in the Introduction to his work against the historic character of the Pentateuch, as his authority for "referring especially to the circumstance, well known to all geologists, that volcanic hills exist of immense extent in Auvergne and Languedoc, which must have been formed ages before the Noachian deluge," &c. Now, instead of this being the fact, I find from the supplement to Dr. Daubeny's *Description of Volcanoes* (p. 748), that Mr. Scrope has always disagreed with Dr. Daubeny as to the ages of these volcanic cones ; and it would appear from the article in the *Quarterly Review* I am now about to cite, that at one time Sir Charles Lyell also differed from Dr. Daubeny on this subject ; so that, setting aside the historic evidence altogether—which, as will abundantly appear, has been too much ignored,—it is not a true representation of the facts of the case to allege, as Dr. Colenso has unfortunately ventured to do, that it is a circumstance well known to all geologists, that these cones must have been formed ages before the Noachian deluge !

I shall now proceed to quote from the article in the *Quarterly Review*, which has the title "The Conquest and the Conqueror," and is otherwise well worthy of attentive perusal. (*Quar. Rev.*, vol. lxxiv., No. 148.)

Referring to the probability that the fires of Vesuvius might have been "quenched before the soil of Italy had been trod by the sons of Japhet," up to the time when they again burst forth in the days of Pliny ; and referring to the remarkable omission of all allusion by that precise writer to the destruction of Herculaneum and Pompeii, the reviewer goes on :—

"Concerning the destruction of Herculaneum and Pompeii, Pliny says nothing ; an omission so singular, that, as Mr. Lyell truly says, it baffles all explanation. Nor is the void of Pliny's information otherwise than most scantily supplied by the sources which might have been expected to afford us aid. Amongst the whole body of Greek and Roman writers, three only notice the entombment of these polluted communities. Our knowledge of a visitation such as no human being had beheld since the destruction of the cities of the plain, is derived merely from the casual allusion of the epigram-

matist, the confused hint of Tacitus,—‘*Haustæ aut obrutæ urbes fecundissimâ Campaniæ orâ*,’—and the tradition reported by Dion Cassius. Had Herculaneum and Pompeii never been discovered, the accounts transmitted to us of their tragical end, would therefore have been discredited by the majority of critical inquirers, so vague and general are the narratives, or so long subsequent to the event. Mr. Lyell thereupon wisely observes, ‘This case may often serve as a caution to the geologist, who has frequent occasion to weigh in like manner negative evidence derived from the silence of eminent writers, against the obscure but positive testimony of popular tradition.’

“Perhaps even more remarkable than the record of the first outbreak, within the historical period, of volcanic activity in the Italian peninsula, are the circumstances attending the memorials of the last known occurrence of such phenomena in Central France. During three years (458—460), Auvergne and Dauphiné were convulsed by violent and continued volcanic eruptions ; streams of lava bursting forth from the summits of the mountains, broke down the cones which ejected continuous ignited showers, attended by earthquakes, shaking, as it were, the foundations of the earth. Thunders rolled through the subterranean caverns ; so awful were the concussions, the sounds, the fires, that the beasts of the forest, driven from their haunts, sought refuge in the abodes of mankind. Strange as it may seem, these phenomena are commemorated by the usages of the Church, and inscribed in the pages of our Liturgy.

“An impending invasion of the Goths added to the terror of the threatenings of nature. Instructed and profiting by the example of the Ninevites, Mamertus, bishop of Vienne, assembled his people in prayer and humiliation. To avert the evil, he instituted the solemn Litanies, or Rogations, on the three days immediately preceding the feast of the Ascension, and which three days acquired distinctively the appellation of *Rogation Days*, because they were the only days of the year then annually set apart for the purpose of such solemn supplications. These forms of prayer, rendered more impressive by the awful character of the calamities and portents which had suggested them, corresponding so nearly with the signs and judgments of Scripture, were speedily adopted throughout Gaul and England. Here they were continued by usage and tradition, until finally established as a portion of the national ritual, in the council held at Cleofeshoe (A.D. 749), which appointed that those three days should be kept holy, after the manner of former times ; and it is hardly needful to observe that the Rogation Days retain their station in the rubric of the Church of England at the present day.

“A remarkable epistle of Sidonius Apollinaris, Bishop of Clermont (consecrated A.D. 471), who resided on the borders of the Lake Aidat, which owed its existence to the damming up of a river by a lava-current, addressed to Mamertus himself, within fourteen or at most sixteen years after the events (Mamertus died A.D. 474), preserves a full notice of the earthquakes and the volcanic eruptions, the crumbling of the cones and the heaping of the showers of ashes and scoriæ cast forth amidst their fires. Alcimus Avitus, the successor of Mamertus, carries on the chain of testimony. This prelate, who was promoted to the see of Vienne about thirty years after the eruptions (A. D. 490), composed an ample series of Rogation Homilies ; and, in addressing his people, he recalls to their memory the events which a great portion of them must have witnessed, and exhorts them to gratitude for the deliverance they had received. These homilies appear to have been numerous ; but, with the exception of some fragments, all have been lost except two ; and amongst the strange examples of the oblivion attending written evidence, not merely when lurking in archives or concealed in manuscripts, but when amply diffused by means of the printing-press, we may remark that this is perhaps the

first time that Avitus has been quoted as elucidating either Sidonius, or Gregory of Tours, the latter of whom also notices the events, though with more brevity.

"An eminent geologist, forgetting Mr. Lyell's sensible prohibition against entertaining arguments deduced from the silence of historical authorities, and zealously anxious to assert the wholesome doctrine of the indefinite antiquity of the Auvergne volcanoes, apparently contradicted by the freshness of their aspect, exhorts us to reject the evidence of our senses, in order to support a theory sustained only by negative proof. He desires us to remark that Julius Cæsar, who encamped in their vicinity, could scarcely have failed to notice them. Yet has not the writer's enthusiasm caused him to forget that the *Mont d'Or* may have been, like Vesuvius, in a state of temporary quiescence: and, in the case of a military commander, whose main object was the narrative of his operations, should not the inquirer peculiarly avail himself of Mr. Lyell's caution against drawing inferences from silence? Again, the geologist appeals to the absence of any mention of these volcanoes in the great work of the Roman naturalist; yet here again is not the deduction overstrained? In one chapter of fifteen lines, the elder Pliny enumerates the cities of Aquitaine, and does nothing more. Had he possessed a full record of the eruptions, would his omission of facts known to him only by report, have been more remarkable than the neglect of the younger Pliny to notice the fiery burial of the cities which took place in his immediate presence? And if the list of Gaulish eruptions, occurring during the most calamitous and disturbed era of the declining empire, when, in Gaul, we have literally no historians or chroniclers at all, had been utterly uncommunicated in the written page, we could not have been surprised at the absence of the information required.

"Yet the testimony has been given to us. In this dark and obscure era two witnesses rise from the tomb, not men of obscure station or humble authority, but individuals of the highest rank, concerning whose character and respectability, if such a term can be employed, we are as fully convinced as if they were living at the present day. Sidonius, the poet, the prefect, the patrician, the senator, the bishop; Alcimus Avitus, equally high in the Church, nephew of an emperor, counsellor and friend of Clovis, the founder of the Frankish monarchy. These, not recording the events in the studied chronicle, or in the technical description of the naturalist, or the exaggeration of the poet, but in the language of friendship and devotion. Briefly and emphatically they advert to transient calamities as the reason for lasting gratitude and repentance; speaking not to strangers who would need any elaborate explanations of localities, nor preserving details to satisfy the curiosity of posterity, but seeking the comfort and edification of the friends and contemporaries whom they addressed,—men who had seen the incandescent streams and showers, heard the subterranean thunder, felt the earth shake beneath their feet, knelt before the same altar, uttered the same prayers,—the people to whom every word and every expression of the preacher brought up in their minds the whole spectacle of the desolation which had mercifully passed away."

The able Reviewer here appends the following foot-note :—

"The observation in our text respecting the claim to 'indefinite antiquity' possessed by the Auvergne volcanoes, as evidenced by Cæsar and Pliny, are those of Dr. Daubeny (*Daubeny on Volcanoes*, p. 14, quoted by Mr. Lyell, *Elements of Geology*, ii. 305); but Dr. Daubeny mistakenly ascribes the same silence to Sidonius Apollinaris; whilst, singularly enough, the very witness upon whose omissions the geologist lays the most stress, is the

one who is the most explicit. We add as large extracts from the epistle of Sidonius and the homily of Avitus as we can find room for. But those who are interested in the subject will do well to consult the originals. In considering the words of these *speakers* rather than writers, it must be recollected that though other notices of the phenomena are merely incidental, and not purposely descriptive or historical, *yet that they are far more ample, intelligible, precise, and correct than Tacitus, who omits all notice of Vesuvius or the eruption, in his account of the Campanian cities.*

“‘Sidonius, Domino Papæ Mamerto salutem :—Rumor est Gothos in Romanum solum castra movisse. Huic semper irruptioni nos miseri Arverni janua sumus. Namque odiis inimicorum hinc peculiaria fermenta subministramus, quia quod necdum terminos suos ab oceano in Rhodanum Ligeris alveo limitaverunt, solum sub ope Christi moram de nostra tantum obice patiuntur. Circumjactarum vere spatia tractumque regionum jam pridem regni minacis importuna devoravit impressio. Sed animositati nostræ tam tenerariæ, tamque periculosæ, non nos aut ambustam murorum faciem, aut putrem sudium cratem, aut propugnacula vigilum trita pectoribus confidimus opitulatura : solo tamen invectarum te autore Rogationum palpamur auxilio : quibus inchoandis, instituendisque populus Arvernus, et si non effectui pari, affectu certe non impari cepit imitari, et ob hoc circumfusus necdum dat terga terroribus. Non enim latet nostram sciscitationem, primis temporibus harumce supplicationum institutarum, civitas cælitus tibi credita *per cujusmodi prodigiorum terriculamenta vacuabatur.* Nam modo, *scenæ mœnium publicorum crebris terræ motibus concutiebantur ; nunc, ignes sepe flammati caducas culminum cristas superjecto favillarum monte tumultabant ; nunc stupenda foro cubilia collocabat audacium pavenda mansuetudo cervorum : cum tu inter ista, discessu primorum populariumque statu urbis exinanito, ad nova celer veterum Ninivitarum exempla decurristi, ne divinæ admonitioni tuæ quoque desperatio convitiaretur.*—(Sid. Apollinaris Epi. vii. 1, Ed. Sirmond. i. p. 586.)”

“The title *Pope* is given to Mamertus by the elder writers, and perhaps the style of *Pope* was assumed by or given to the see of Vienne, so venerable for its antiquity. We must now take Avitus :—‘Currit quidem tramite vitali, non per Gallias tantummodo, sed pene per orbem totum Rogationalis observantiæ flumen irriguum, et infectam vitis terram uberi fluxu annuæ satisfactionis expurgat. Peculiarior tamen nobis in hac ipsâ institutione servitii et gaudii causa est ; quia quod hinc modo ad cunctorum utilitatem defluit, ex nostro primitus fonte manavit : et forte nunc pertineat ad cujuscumque privilegii ornatum sumptæ primitus institutionis exordium.

“‘Ceterum cum ad hujusmodi humilitatem ineffabilis necessitas rigida Viennensium corda perdomuit, sentiens ecclesia nostra causam ægritudinis suæ, non sibi quasi maxime præ omnibus, sed quasi soli ex omnibus, existimans opus esse instituendâ observatione præsentis, sollicitius captavit remedium quam primatum.

“‘Et quidem terrorum temporis illius causas multos nostrum recolere scio. Siquidem *incendia crebra, terræ motus assidui, nocturni sonitus, cuidam totius orbis funeri prodigiosum quoddam bustuale minitabantur.* Nam populosis hominum concursibus domestica silvestrium ferarum species observabatur, Deus viderit an ludificans oculis, an adducta portentis. Quicquid tamen ex iis duobus foret, perinde monstruosum intelligebatur, seu sic veraciter immania bestiarum corda mansuefieri, seu tam horribiliter conspectibus territorum falsæ visionis phantasmata posse confingi. Inter sentiebant dissimulando, quæ fletui nolebant dare, casui dabant ; alii spiritu salubriore, abominabilia nova quoque congruis malorum proprietatis significationibus interpretabantur. *Quis enim in crebris ignibus, imbres sodomiticos non timeret ? Quis trementibus elementis, aut decidua culminum, aut disrupta*

terrarum imminere non crederet? Quis videns, certe videre se putans, pavidos naturaliter cervos per angusta portarum usque ad fori lata penetrantes, non imminensem solitudinis sententiam formidaret?—(Alcimi Aviti Homilia de Rogationibus, ed. Sirmond., ii. 90.)”

The above article in the *Quarterly Review* was published in October, 1844. In 1858, also, Mr. Scrope “brought out a new edition of his beautiful work, *On the Geology and Extinct Volcanoes of Central France*, in which he denied altogether the correctness of the division which Dr. Daubeny had proposed for the volcanoes of Auvergne into modern and ancient.” So we are informed by Dr. Daubeny himself in the *Supplement* to his History, before referred to, copies of which were distributed at the meeting of the British Association at Cambridge in 1862. In it Dr. Daubeny admits “*that the eruptions which he had designated as ancient are not divided, in point of time, from the so-called modern ones, by any great deluge or cataclysm which overspread the country,*” though he still maintains that there is, “generally speaking, a marked difference in the volcanic products of Central France, *in correspondence with their relative antiquity,*”—a somewhat vague qualification of the previous more definite admission; and were it of much consequence for my present argument, I might show by more ample citations, that these qualifications are based upon assumptions of antiquity merely, which again are based partly upon the old abandoned theory of igneous formations, and partly upon the appearances that are assumed to favour “the distinction between lavas of submarine and subaërial origin.” But I make the following extract, as bearing on the present question, and also upon the now presumed great antiquity of man, since man’s contemporaneous existence with certain extinct animals has been discovered :—

“I have omitted, in my account of the rocks of the Puy, all mention of those remarkable accumulations of scorïæ which occur at Mont Denise, and at other places near the town of Puy, evidences of volcanic action of the most recent epoch.

“It was underneath the scorïæ which caps Mont Denise that Mr. Scrope mentions the occurrence of a volcanic breccia or peperino, which, though of great antiquity, as shown by being antecedent to the excavation of the valley which it overlooks, has been found to contain human skeletons, associated with bones of the elephant, rhinoceros, cervus elephas, and other large mammalia. If this be fully substantiated, it would lead to the inference that man must have existed long before the volcanic eruptions of the country had reached their termination.” (*Suppl.*, pp. 749, 750.)

I must here notice the assumption of man’s great antiquity, in the above extract, depending upon the supposed “great antiquity” of the formation in which the remains were found; and (as M. Prestwich said with reference to the flint implements found at Amiens) the evidence here also may yet be found “as much to necessitate the bringing forward the extinct animals towards our own time as the carrying back of man to the geological times.”*

* Vide *Journ. of Trans. of Vict. Inst.*, vol. i. p. 34.

In the article in the *Quarterly* already cited will be found further evidence to this effect and in support of Mr. Scrope's views.

But, to resume. Notwithstanding the long-standing difference of opinion between Dr. Daubeny and Mr. Scrope as regards the antiquity of these extinct volcanic cones, and notwithstanding Sir Charles Lyell's former caution as exhibited in the above citations, he now takes for granted their great age, disregards altogether the historic evidence of their recent eruption, and merely argues from the *quasi* facts against the universality of the Deluge. In his *Antiquity of Man* (p. 192) he says :—

"We behold in many a valley of Auvergne within fifty feet of the present channel, a volcanic cone of loose ashes, with a crater at its summit, from which powerful currents of basaltic lava have poured, usurping the ancient bed of the torrent. By the action of the stream, *in the course of ages*, vast masses of the hard columnar basalt have been removed, pillar after pillar, and much vesicular lava, as is the case, for example, of the Puy Rouge, near Chalucet, and of the Puy de Tartaret, near Neckers. . . . *Had there been a single flood fifty or sixty feet in height, since the last eruption occurred, a great part of these volcanoes must inevitably have been swept away.*"

In his *Principles of Geology*, also, chap. 45, he says :—

"We may be enabled to infer, from the integrity of such conical hills of incoherent materials, *that no flood can have passed over the countries where they are situated since their formation.*"

Now, this is very valuable testimony by Sir Charles Lyell (supposing his conclusion to be sound), that no flood of water can possibly have covered these volcanic cones since they were originally erupted. His reasoning upon this point, however, has been controverted ; for instance by the Rev. James Brodie, in his *Remarks on the Antiquity and Nature of Man, in Reply to Sir Charles Lyell* ;* for he thinks these mountain cones of Auvergne might "have been sunk once and again beneath the deep without a single cinder having been moved." (p. 42.) Sir Charles's and Dr. Daubeny's conclusions as to the great antiquity of the fossil remains there discovered have also been questioned by other geologists, on independent grounds. For instance, Mr. J. R. Pattison, F.G.S., in his *Examination of Sir Charles Lyell's Antiquity of Man*,† thus writes :—

"The testimony of the fossil man of St. Denise (if credit can be given to it, which, from personal inspection, I think is the case) proves merely an antiquity equal to that of the cave-remains. The specimen is embedded in a breccia which resulted, as M. Aymard concludes (*Congrès Scientifique de France*, 22me Session) from a volcanic eruption of water amidst scorïæ at the very close of the volcanic period in Velay, after the surface had attained nearly its present contour, and whilst extinct and subsisting species of mammals inhabited Auvergne."

I think I need make no further citations to establish the fact that there is no unison or agreement among geologists, and never has been, as regards

* Lond., Hamilton, Adams, & Co., 1864.

† Lond., Lovell Reeve & Co., 1863. (2nd ed., p. 15.)

these various controverted points. And yet let us see how Dr. Colenso has satisfied himself, and endeavours to satisfy others, upon a question that, even having regard to geological evidence alone, is full of doubt and difficulty. He says :—

“My own knowledge of some branches of science—of geology in particular—had been much increased since I left England ; and as *I now know for certain, on geological grounds*, a fact of which I had only misgivings before, viz., that a *universal deluge, such as the Bible manifestly speaks of, could not possibly have taken place* in the way described in the Book of Genesis. I refer especially to the circumstance, *well known to all geologists* (see Lyell’s ‘Elementary Geology,’ pp. 197, 198), that volcanic hills exist of immense extent in Auvergne and Languedoc, *which must have been formed ages before the Noachian deluge*, and which are covered with light and loose substances, pumice-stone, &c., *that must have been swept away by a flood*, but do not exhibit the slightest sign of having ever been so disturbed. Of course (he adds), I am well aware that some have attempted to show that Noah’s deluge was only a partial one ; but such attempts have ever seemed to me to be made in the very teeth of the Scripture statements, which are as plain and explicit as words can possibly be.”

I have drawn attention to the extremely positive character of Dr. Colenso’s opinions and assertions, as regards his assumed knowledge of geological “facts,” by putting some of his sentences in italics. He appears totally unaware of the historic evidence bearing upon the whole question ; and, as he throws over the Pentateuch, he probably shares with Sir Charles Lyell the opinion that “*true history and chronology are the creation, as it were, of yesterday*. Thus the first Olympiad is generally regarded as the earliest date on which we can rely, in the past annals of mankind,—only 772 years before the Christian era.” (*Antiq. of Man*, p. 380.)

Those who read this note may feel inclined to doubt whether “*true history*” can be confidently reckoned upon even to-day ! It is not, every one must be convinced, always very well treated even when within our reach. I hesitate to express in my own words all I feel as regards what is either the ignorance or obliviousness that has been exhibited with reference to the historic evidences of the date of the volcanic eruptions in Auvergne. The state of the case is put briefly thus in Archdeacon Pratt’s *Scripture and Science not at Variance*.*

“Some years ago, a geological lecturer of no ordinary note [Dr. Daubeny] asserted that the volcanoes of Auvergne, in Central France, have not been in activity for many ages—certainly not since the days of Julius Cæsar, who pitched his camp there in perfect safety ; and he took the intervening period of nearly 2000 years as the first step for measuring the antiquity of the deposits in those parts. Whereas, ten or twelve years subsequently [*i. e.* subsequent to the Lecturer’s assertion] an old Gaulish history was re-edited, from which it appears that during three years, long after Julius Cæsar, viz. in A.D. 458-460, the district was convulsed with violent and continued eruptions, and streams of lava carried destruction before them. (*Quar. Rev.*, Oct. 1844.)”

* Lond., Hatchard & Co., 5th ed., 1864. (p. 160.)

It is to be observed that the so-called "geological grounds" upon which the notion of the antiquity of these mountain cones was based, were not *geological* at all. It was based upon ignorance, mistaken for knowledge, and miscalled "negative evidence" *from history*. Julius Cæsar did not notice that the mountains were in a state of eruption; *therefore* they were not then, *nor afterwards*, in an active state! And of course some may say, if history was entirely silent on the subject, it was not unnatural to conclude that the eruptions must have taken place a very long time ago. In reply to this I would observe that people might very easily *guess* something of the sort. But it is worse than ridiculous to call such guessing science, and to talk of such opinions being founded upon "geological grounds."

But then comes the confounding fact, that history has been far from silent on the subject. Not only so; but I am now about to show that the historic proof of the volcanic eruptions in Auvergne has by no means depended upon the re-editing of Sidonius or Avitus in our own day, but has been kept up on the face of history, sufficiently, at least, to have prevented any moderately well-read English theologian from following the geologists blindly in their erroneous path. The origin of our "Rogation Days" has been shown by the able writer in the *Quarterly Review* to be traceable to the violent eruptions of the volcanoes of Auvergne in the fifth century. But Dr. Colenso might have easily found that out long before 1844. He will find the fact sufficiently referred to in Nelson's *Companion for the Festivals and Fasts of the Church of England (in loco)*, where Le Comte's *French Ecclesiastical Annals* are cited in confirmation of the text, and will probably narrate what were the precise "calamities" merely so referred to in Nelson. A still more definite reference to these calamities is to be found in Hooker's *Ecclesiastical Polity* (Book V. ch. xli. §§ 1-4). Cartwright, whom he is answering, had objected to the prayers in our Litany against "dangers which are nothing near us,"—lightning, storm and tempest, &c., and refers thus to the origin of what he calls "this abuse" in the Church:—

"There was one Mamercus, Bishop of Vienna [Vienne], which in the time of great earthquakes which were in France, instituted certain supplications, which the Grecians (and we of them) call the Litany," &c.

Hooker, in reply, after noticing that what the Greek Church termed Litanies were called Rogations of the Latins, then goes on to say:—

"To the people of Vienna (Mamercus being their bishop, about 450 years after Christ) there befell many things, the suddenness and strangeness whereof so amazed the hearts of all men, that the city they began to forsake as a place which heaven did threaten with imminent ruin. It besemed not the person of so grave a prelate to be either utterly without counsel, as the rest were, or in a common perplexity to show himself alone secure. Wherefore, as many as remained he earnestly exhorteth to prevent portended calamities, using those virtuous and holy means wherewith others in like case have prevailed with God. To which purpose he perfecteth the Rogations or Litanies before in use, and addeth unto them that which the present necessity required. Their good success moved Sidonius, Bishop of Aversa, to use the same so

corrected Rogations (*Sidon.*, lib. vii. Epist. i.—ad Mamercum), at such time as he and his people were after afflicted with famine, and besieged with potent adversaries."

I need not, however, make further citations from Hooker, who explains the connection between these "Rogations" and the Rogation Days established by the Council of Aurelia, A.D. 506, and also with the petitions in the Litany of the Church of England against sudden calamities, to which Cartwright had objected. The Oxford edition of Hooker's works (1845), from which I quote, refers also to Palmer's *Origines Liturgicæ*, i. 267-272, where, also, these "dreadful calamities" are referred to, thus affording a sufficient key to this neglected passage of "true history"—well-nigh forgotten, though twelve centuries later than the First Olympiad!

And what is the brief sum of the whole matter as regards the extinct volcanoes of Auvergne? Supposing Sir Charles Lyell to be right in his conviction that these mountain cones have never been covered with water since they were last erupted, then that certainly would prove that they were not erupted prior to the general deluge. But, instead of that conclusion supporting Dr. Colenso's illogical scepticism, that *therefore* the deluge was not *universal*, as the Bible "manifestly" teaches, it merely confirms the modern historical evidence that the eruptions took place not only long after Noah's flood, but even long after Julius Cæsar invaded Gaul,—namely, in the fifth century of our era. Thus the sacred history of the universal deluge is not affected by what "we know for certain" respecting Auvergne; and one of the most positive geological conclusions of Sir Charles Lyell, that these mountain cones were never under water, is confirmatory of the historic evidence, which every time we hear the Litany in church, and as often as the Rogation Days before Ascension-tide come round—as often, even, as we see the boys of a parish "beating the bounds," that old custom being in fact a relic of the Auvergne processional Rogations,—must now be brought to our remembrance, to remind us of this extraordinary specimen of "the Logic of Scepticism:" with which Dr. Colenso has "especially" identified himself. One thing is completely settled besides, by the whole evidence now before us,—namely this, the *very modern character* of those mountain cones, the fancied great antiquity of which was first guessed at, and then put forward as established upon "geological grounds," and lastly relied on as one of the grand proofs of the antiquity of man in connection with his probable ape origin!—Verily, "*pulchræ illæ meditationes et speculationes humanæ et causationes res malisana sint, nisi quod non adsit qui advertat!*" (*Nov. Org.*, Aph. X.) I may add, that the age of the deposits in the valley of the Somme is also affected by this disproof of the antiquity of the Auvergne mountain cones; inasmuch as the "flint implements" there found were embedded with palæontological remains, similar to those discovered along with the "fossil man of St. Denise."

ANNUAL GENERAL MEETING, MONDAY, MAY 27, 1867.

THE REV. WALTER MITCHELL, VICE-PRESIDENT, IN THE CHAIR.

THE CHAIRMAN.—Our proceedings at the First General Meeting of the Institute, held on the 24th May last year, being printed in our *Journal of Transactions*, the first business I have to bring before this meeting is, to submit for approval the First Annual Report of the Council, which I now request the honorary secretary to read to you.

MR. REDDIE then read the Council's report, as follows :—

*FIRST ANNUAL REPORT of the Council of the
VICTORIA INSTITUTE, OR PHILOSOPHICAL SOCIETY OF
GREAT BRITAIN.*

Progress of the Society.

1. THE COUNCIL have much satisfaction in laying before the Members and Associates of the VICTORIA INSTITUTE their First Annual Report of the progress and operations of the Society during the first year of its existence. At the first General Meeting of the Institute, held on 24th May, 1866, to inaugurate its proceedings, the Provisional Committee reported that 192 Members and Associates had joined. Since then, 99 Members and Associates have been added, up to the 1st of this month, according to the printed lists now before the meeting, consisting of the following various classes, viz. :—

On the Foundation Lists.

2	Vice-Patrons and Life Members.
13	Life Members.
227	Annual Members.
3	Life Associates, 2nd class.
13	1st class Associates.
24	2nd „ do.

282 Total on the Foundation Lists.

Additional for 1867.

3	Members.	
2	1st class Associates.	
4	2nd „	do.

291 Total to 1st May, 1867.

2. The Council regret to state that the Institute has lost two of its original Members by death, and that there have been six withdrawals, some of which, however, it is hoped, may not be final; as, with one exception, they have been accompanied with expressions of regret at leaving the Society, and of continued sympathy with its objects, and were not sent in till after the commencement of the present year.

Finance.

3. The Balance-Sheet of the TREASURER is appended to this Report, showing the actual Receipts and Expenditure for the past year.

4. Taking the numbers upon the Foundation Lists, the total assets for the year ending 31st Dec., 1866, amount to £959. 14s. 0d. Of this amount, the sum of £430. 10s. accrued from the donations of the Vice-Patrons, Life Members, and Life Associates, and has been, or will be, funded; leaving the balance of £529. 4s. as the amount of annual subscriptions, or the ordinary income of the Institute. For the present year (1867), taking the annual subscribers standing upon the lists on 1st May (and omitting the names above referred to as possible withdrawals), the assets will be as follows:—

219	Foundation Members, at £2. 2s.	...	£459	18
3	Members, at „	...	6	6
	Do., Entrance Fees	...	3	3
15	1st Class Associates	...	31	10
28	2nd „ Do.	...	29	8
<hr/>				
265			£530	5
18	Vice-Patrons, Life Members, and Life Associates.			
<hr/>				
283	Total.			

5. This income is quite sufficient to meet the expenditure of the Institute, so far as the Council can yet venture to endeavour to carry out its Objects. Convenient apartments as offices, and for holding the meetings of the Society, have been

secured, on moderate terms, from the Architectural Union Institute; and the primary objects of the Society have been already successfully advanced by the various papers read and discussed at the ordinary meetings of the Institute. But it must be obvious that before Objects 6 and 7 can be hoped to be undertaken or realized, there must be a large accession of numbers, and an increase of the funds of the Society, and that thoroughly qualified paid officers must be employed to aid in carrying out these objects to the full extent contemplated. At present there is only one paid officer of the Society, Mr. C. H. H. Stewart, who is engaged as clerk at a moderate salary.

Meetings.

6. Sixteen Ordinary Meetings have been held since the inauguration of the Victoria Institute on 24th May last year, and two more will be held next month, to complete the Session for 1866-67. The following are the titles of the papers for each of those meetings, and the authors' names, viz. :—

- A Sketch of the Existing Relations between Scripture and Science. By GEORGE WARINGTON, Esq., F.C.S., M.V.I. (Read and discussed June 4th, 1866.)
- On the Difference in Scope between Scripture and Science. By the late C. MOUNTFORD BURNETT, Esq., M.D., Vice-President V.I. (June 18th.)
- On Comparative Philology. By Rev. ROBINSON THORNTON, D.D., Head Master of Epsom College, M.V.I. (July 2nd.)
- On the Various Theories of Man's Past and Present Condition. By JAMES REDDIE, Esq., Hon. Sec. V.I. (July 16th.)
- On the Language of Gesticulation and Origin of Speech. By Professor J. R. YOUNG, M.V.I. (19th Nov.)
- On Miracles; their Compatibility with Philosophical Principles. By the Rev. W. W. ENGLISH, M.A., M.V.I. (3rd Dec.)
- Thoughts on Miracles. By E. B. PENNY, Esq., M.V.I. (3rd Dec.)
- On the General Character of Geological Formations. By EVAN HOPKINS, Esq., C.E., F.G.S., M.V.I. (17th Dec.)
- On the Past and Present Relations of Geological Science to the Sacred Scriptures. By the Rev. Professor JOHN KIRK, M.V.I. (7th Jan.)
- On the Lessons taught us by Geology in relation to God. By the Rev. J. BRODIE, M.A., M.V.I. (21st Jan.)
- On the Mutual Helpfulness of Theology and Natural Science. By Dr. GLADSTONE, F.R.S., M.V.I. (21st Jan.)
- On Falling Stars and Meteorites. By the Rev. WALTER MITCHELL, M.A., Vice-President V.I. (4th Feb.)
- On the Probable Ages of the Continents, founded upon Astronomical Data and Geological Facts. By EVAN HOPKINS, Esq., C.E., F.G.S., M.V.I. (18th Feb.)

- On the Credibility of Darwinism. By GEORGE WARINGTON, Esq., F.C.S., M.V.I. (March 4th, March 18th, and April 1st.)
- On Utilitarianism. By JAMES REDDIE, Esq., Hon. Sec. V.I. (April 15th.)
- On the Logic of Scepticism. By the Rev. ROBINSON THORNTON, D.D., M.V.I. (May 6th.)
- On the General Isomorphism of all Crystalline Bodies, and the Relation of all Forms of Crystals to those of the Cubical System. By the Rev. WALTER MITCHELL, M.A., Vice-Pres. V. I. (To be read June 3rd.)
- On the Relations of Metaphysical and Physical Science to the Christian Doctrine of Prayer. By the Rev. Prof. JOHN KIRK, M.A., M.V.I. (June 17th.)

7. The meetings have been well attended, and generally very great interest has been taken in the papers read and in the discussions that have followed, which have been fully reported in the *Journal of Transactions*, in accordance with Object 4. This is, no doubt, attended with considerable expense; but the advantages are undeniable, and the Council have reason to believe that the Members generally have felt the great importance of the printed discussions in furthering the best interests of the Institute.

Publications.

8. The Papers read between 4th June, 1866, and 4th February, 1867, with the discussions thereon, have now been published in the first four numbers of the *Journal of Transactions*, completing Vol. I. thereof. The remaining papers of the Session, with the discussions upon them, will be published in due course in subsequent numbers of the Journal, each number being published, as nearly as practicable, every quarter.

9. In addition to these regular publications of the *Journal of Transactions*, since the Inaugural Meeting, the Council have authorized the separate publication of the Rev. Professor Kirk's admirable *Discourse on Geological Theories*, read before the Society on the 7th of January last, mainly with the view of making the objects and practical working of the Society known, and to obtain new members; the pamphlet, *Scientia Scientiarum*, and the Vice-President's *Inaugural Address* having been in like manner distributed, and sold separately, for the same purposes. Separate copies of each paper read before the Society, with the discussions thereon, have also been thrown off to a limited extent, partly for the authors of the papers, and partly for gratuitous distribution in likely quarters, to make the work of the Institute better known.

10. The Council feel that they may confidently appeal to the *Journal of the Transactions*, so far as published, as an ample

justification of the institution of the Society, and as the best evidence of its importance, and of the success which has attended its earliest operations.

Regulations and Bye-laws.

11. It was intended to have summoned a Special General Meeting previous to the present, for the purpose of agreeing to the Regulations relating to the Objects, Constitution and Bye-laws of the Institute. The pressure of other business, however, has prevented this being done, and the Bye-laws are now therefore submitted for the consideration and adoption of the present General Meeting, as printed and approved by the Council.

12. The Council desire that they may be permitted to add to the Foundation Lists of the Institute the names of new Members and Associates, upon such terms as they may deem advisable; several names having already been added, although enrolled after 31st December last, at the candidates' earnest desire, and upon their paying the same subscriptions as the Members who had joined prior to that date.

Conclusion.

13. The Council trust that the success which has attended the first year of the Society's existence will only serve as a stimulus to the Members and Associates to exert themselves, in order to secure the further accession of its numbers and continued prosperity. The addition of new and active members, who will take part in our proceedings and contribute papers, is of the greatest importance. If every Member and Associate would only induce one other to join, our numbers would quickly be doubled; and it is mainly by such individual exertions, and the co-operation of those gentlemen who have kindly engaged to act as Honorary Local Secretaries, that the objects of the Victoria Institute can be made known and fully attained.

Signed on behalf of the Council,

WALTER MITCHELL, Vice-President,
Chairman.

The CHAIRMAN.—I now beg to call upon the Treasurer to read the first balance-sheet.

The balance-sheet was then read by Capt. Fishbourne, as follows :—

FIRST ANNUAL BALANCE SHEET, from 24th May, 1865, to 31st December, 1866.

RECEIPTS.		£.	s.	d.	EXPENDITURE.		£.	s.	d.
1 Vice-Patron and Life Member		63	0	0	To Wyman and Sons, for Printing		170	18	0
9 Life Members at £21 each		189	0	0	Warrington, for Printing		12	11	8
187 Annual Members, at £2. 2s. each		392	14	0	Ortner and Houtle, for Engraving		9	15	11
11 Associates (1st class) at £2. 2s. each annually		23	2	0	Hardwicke, for Scientia Scientiarum		26	2	8
3 Life Associates (2nd class) at £10. 10s. each		31	10	0	Salary of late Assistant Secretary (6 months)		50	0	0
22 Associates (2nd class) at £1. 1s. each annually		23	2	0	" present Clerk (6 months)		26	0	0
1 Ditto, Subscription for 1867		1	1	0	Giles, for Reporting Meetings from June 4 to July 5		10	10	0
A Subscriber (per J. J. Lidgett, Esq.)		1	1	0	<i>London Mirror</i>		4	3	0
Journals sold at Office		0	10	0	Chas. Tenpenny, for Rent, at St. Martin's Hall		10	10	0
					Mr. Humphries, for Rent, at 32, Sackville Street		5	0	0
					Lloyd, for Furnishing Office, 9, Conduit Street		19	0	6
					Postages, for Journals, &c.		30	12	5
					Advertising		14	7	6
					Disbursements made by Clerk for Office Expenses, }		15	13	7
					such as fuel, paper, &c.		3	3	0
					Hire of Piano, and two Singers at Dinner		13	13	0
					Dinner Tickets to Editors and Musicians		4	10	7
					Mrs. Wilkins, for Refreshments at meetings, &c. ...		251	10	0
					Invested in New Three per Cent. Annuities		0	10	0
					In hands of Treasurer		0	7	6
					Balance in Petty Cash-box		46	0	8
					" at Bank				
							£725	0	0
Balance in the Bank					Brought forward				
Subscriptions for 1866, since paid :—					Subscriptions for 1866, still due :—				
1 Vice-Patron and Life Member		46	0	8	1 Life Member		200	7	8
3 Life Members		63	0	0	27 Annual Members		21	0	0
13 Annual Members		27	6	0	1 Associate (1st Class)		56	14	0
1 Associate (2nd Class)		1	1	0	1 Associate (2nd Class)		2	2	0
							1	1	0
							£281	4	8
Carried forward ...					Examined and found correct.—J. J. LIDGETT, } Auditors. W. VANNER, }				

Captain COOPER GARDINER then moved the following resolution :—"That the report of the Council be adopted, printed, and circulated."

Professor MACDONALD.—I have much pleasure in being able to second the motion that has been made for the adoption of the report; and I trust that the warmest anticipations of our excellent Chairman on behalf of the Council will be fully realized, and that we may have an increased flow of members to the Institute. (Hear, hear.)

The resolution was put to the meeting and carried unanimously.

The CHAIRMAN.—The Objects, Constitution, and Bye-laws of the Institute having been carefully considered by the Council, and printed copies having been sent to every member and associate, I think it will not be necessary to read them over now; so, with your kind permission, I shall take them as read; and I may explain, that if now approved and adopted, they will be printed in the appendix to the first volume of our *Journal of Transactions*, which will shortly be completed.

JOHN COLEBROOK, Esq.—I have much pleasure in moving—"That the Objects, Constitution, and Bye-laws of the Victoria Institute or Philosophical Society of Great Britain, as drawn up by the Council and printed, be now adopted."

G. CRAWFURD HARRISON, Esq., seconded the resolution, which was carried unanimously.

[The Objects, Constitution, and Bye-laws, as now adopted, will be found printed in Vol. I. of the *Journal of Transactions*, pp. 476-486.]

WILLIAM STEWART, Esq.—I have much pleasure in moving—That the thanks of the members and associates be given to the Officers and Council of the Institute for the past year.

Professor OLIVER BYRNE said he had great pleasure in seconding the resolution, which was carried unanimously.

PETER ROBERTSON, Esq.—I beg leave to move that the following gentlemen be appointed as the Officers and Council of the Victoria Institute for the ensuing year :—

OFFICERS AND COUNCIL FOR 1867-68.

President.

The Right Honourable the Earl of Shaftesbury, K.G.

Vice-Presidents.

Philip Henry Gosse, Esq., F.R.S.

Rev. Walter Mitchell, M.A.

Honorary Treasurer.

Captain E. Gardiner Fishbourne, R.N., C.B.

Honorary Secretary.

James Reddie, Esq., Hon. Mem. Dial. Soc., Edin. Univer.

Honorary Foreign Secretary.

Edward J. Morshead, Esq., H.M.C.S.

Council.

Rev. William Arthur, M.A.
 Robert Baxter, Esq. (*Trustee*).
 Rev. A. De la Mare, M.A.
 R. N. Fowler, Esq., M.A. (*Trustee*).
 William H. Ince, Esq., F.L.S.,
 F.R.M.S.
 John J. Lidgett, Esq., B.A.
 Alex. McArthur, Esq., F.R.G.S.,
 F.A.S.L.
 William M. Ord, Esq., M.B.

Rev. J. B. Owen, M.A.
 Captain F. W. H. Petrie, F.G.S.
 Rev. Robinson Thornton, D.D.
 George Warington, Esq., F.C.S.
 Alfred J. Woodhouse, Esq., M.R.I.,
 F.R.M.S.
 Rev. W. Reynier Cosens, M.A.
 Alfred V. Newton, Esq., F.A.S.L.
 William Vanner, Esq., F.R.M.S.
 S. D. Waddy, Esq., Barrister-at-Law.

Dr. EDWARD HAUGHTON.—I have much pleasure in seconding the resolution. The work already done by this Society, as shown in the numbers of the *Journal* already printed, is such, I am sure, as to commend it to everybody; and we cannot but have confidence in the recommendation of the Council as regards the new names proposed to be joined to theirs, for the future management of the Institute.

The CHAIRMAN.—As the Society is still in its infancy, we have not thought it necessary to remove any old members of the Council, and we have simply retained the previous members of Council, excepting those whose names, unfortunately, are removed by death. I am sorry to say that, within the last few days we have lost a very valuable member of the Council, Mr. Evan Hopkins,—a great loss not only to this Society, but to the scientific world in general. All we have done for next year, is to add the names of a few other gentlemen to those already on the Council.

The resolution was put to the meeting and carried unanimously.

Professor MACDONALD.—I have great pleasure, Mr. Vice-President, in hearing that there is to be a little new blood introduced into the Council; and I hope that the system is not far from being introduced, that there shall be a removal of some names from the Council every year, to be replaced by new members. Circulation, you know, in our profession is the soul of vitality, and unless you introduce that, you are apt to stagnate an Institution.

The CHAIRMAN.—That is quite the intention of the Council, and it is only our state of infancy that has prevented it being at once adopted.

Mr. REDDIE.—I am glad that Professor Macdonald has made these remarks, and I may further explain that our intention as regards the "circulation" necessary to our healthy action, is to drop first the names of those members of the Council who have attended fewest or none of the meetings. It is difficult, of course, to get a new Council into working order; but when we have full numbers, that is what we propose to do. There will be a certain number of members to go out by rotation every year; and we may have a formal resolution at our next annual meeting to that effect. But it is obvious that it might weaken the Society, if at present we were to lose any of those members of Council who have done their work well, as they have done it, during the past year.

Dr. HAUGHTON.—I would make one other suggestion. As, according to the constitution of the Institute, the members have but one occasion in the

twelve months upon which they have an opportunity of making suggestions, I would suggest that they should receive longer previous notice when the meeting is to take place. No doubt we all know that it takes place at a fixed time ; but people may forget that, and be occupied with other things, which may drive the General Meeting of the Victoria Institute out of their heads. It so happens that through some oversight I did not receive notice till Saturday last that this meeting was about to take place. I do not say this, however, to find fault, but I think it desirable that members should in future receive a clear week or ten days' notice of what is the most important meeting of the Victoria Institute during the year.

Mr. REDDIE.—Allow me to explain that we have certainly given more than a week's notice of this meeting to the members generally. Dr. Haughton, residing at Malvern, and having changed his residence there, has unfortunately not received his notice so early, probably on that account, or the post-office may have been at fault ; but at any rate I am glad at the unexpected pleasure of seeing him here. In addition to the individual notices, we have also advertised the meeting ; but in future I shall take care that notices are sent out as early as possible. But I cannot quite account for his circular being so long undelivered.

Dr. HAUGHTON.—I received this only on Saturday ; and I hope you won't think that because members reside in the country, they take less interest in the Institute than members residing in town. In fact, it is mainly to be present at this meeting that I have come 120 miles.

Mr. REDDIE.—I am informed by the clerk, that all the notices were duly despatched to all members in the United Kingdom more than a week ago, but those in town were posted first. I beg leave to observe, that this is just the time when any member who has any suggestion to make as regards the affairs of the Society should do so. It is usual to discuss such matters before the Address is delivered.

Professor MACDONALD.—I may say that I have been more fortunate in getting my notice of this meeting, and I have come 450 miles in order to attend it.

The CHAIRMAN.—I should be happy to hear any suggestions or observations that any member may please to make, before calling upon the Hon. Secretary to read the Annual Address.

Professor MACDONALD.—Would you allow me to suggest to the Council to take into consideration, as you are unable to fulfil at present both the 6th and 7th Objects of the Society, whether at least the 6th Object might not be immediately put in force. I believe nothing would encourage the increase of new members more effectively than the publication of some valuable scientific work bearing upon our objects, by our continental neighbours. I think this would be a far better investment for the funds of the Society than any Joint Stock Company, however limited. (Laughter.)

Mr. REDDIE.—I may explain, as regards the learned Professor's suggestion, that we are most anxious to be able to start with the translation of a book ; but, in the first place, it is difficult to get a volunteer to give us a translation,

and we must as yet also look carefully to the finances of the Society. But if Professor Macdonald will only favour us with a translation of some valuable continental work, we shall give him plenty of time, and I think before he is likely to be ready with it, we shall have funds to carry it through the press, without inconvenience. I may add that I have no doubt that these publications will be remunerative,—and, indeed, I should be sorry if any gentleman engaged in supplying translations should not be well paid for his trouble. And I am also sure of this, that a book ought to have an unlimited, not a limited, circulation, in order to do good; and our intention is, although we shall require funds to start the undertaking, that the authors shall be well remunerated, and in the same way they would be by a publisher for any good book. I trust that by and by the “Imprimatur” of this Society will be as great a recommendation for the books we publish, as the Imprimatur of some other societies might perhaps tend to deter people from purchasing their books.—I will not mention names. (Laughter.)

The CHAIRMAN then called upon the Honorary Secretary, who read the following Address to the meeting :—

ANNUAL ADDRESS.

LADIES AND GENTLEMEN,

Two years ago, on the 24th May, 1865, the proposal to found the VICTORIA INSTITUTE was first put forth, and its foundation laid. Within a year from that date it had gathered such strength, that its first founders saw that their hopes would be realized; and the proceedings of the Institute were publicly and formally inaugurated at the First General Meeting of its Members and Associates, held on 24th May, 1866. Upon that occasion the Inaugural Address was delivered by our Vice-President, the Rev. Walter Mitchell; and I doubt not it has been the general expectation that at the present Anniversary Meeting the First Annual Address would also be delivered by him. I feel, therefore, you may be assured, the great disadvantages attendant upon occupying my present position—the disadvantage of coming after one so highly qualified in every respect to address you upon some of those highest matters of philosophy and science, the consideration of which is the primary object of our association; the dis-

advantage of disappointing you at the outset in thus, I must say, unfortunately being obliged to occupy his place ; and the disadvantage besides of knowing that I can only further disappoint you, in the event, from not being able to speak with his knowledge and his wisdom and his words. I can only plead, in extenuation of undertaking such a task, that I do so not from choice but as a duty. I therefore crave your most kind indulgence ; and I think I may count upon it when I explain, that I address you this evening solely that you may not be disappointed of hearing Mr. Mitchell at our next ordinary meeting on the 3rd of June ;—for he found that to undertake to deliver the Annual Address would so materially interfere with the completion of his *Memoir on the Isomorphism of Crystalline Bodies*, that he could not hope to be able to read it this session, unless he were relieved from the preparation of the Address. Under these circumstances, I venture to trust that you will not only pardon my throwing myself into the breach to fill the place, however unworthily, of our Vice-President, but that you will therefore also accord to me your most favourable consideration. And let me add further, that there is this compensating advantage in our Vice-President being silent on the present occasion : he could not have spoken of himself as I have done, nor told you how much our success last year has been specially due to his many-sided qualifications for presiding over our deliberations, and to his constant and hearty assiduity as our Chairman. Besides you must not suppose that the preparation of the present Address can compare in difficulty with the Inaugural Address last year. We had then no past existence as a society ; our future, though full of hope, was then uncertain ; our work had to be well begun in order to succeed, and you know how well begun it was in that admirable Address. But now we have the work of last year to look back upon and review ; and our existence is not only a fact, but our success has been most signal.

I purpose, therefore, chiefly to occupy your attention with a retrospective summary of the principal subjects we have already discussed in the Victoria Institute. Our first brief session, consisting of four evening meetings in June and July last year, was commenced by a paper, giving *A Sketch of the Existing Relations between Scripture and Science*, by Mr. Warrington, who, in the most impartial manner, detailed the various objections which had been urged in our day, in the name of Science, against the credibility of the Scriptures. I frankly confess that what I may call the severe impartiality

with which Mr. Warington executed this most delicate task caused me for a moment some anxiety. As your Honorary Secretary, you are aware, I have the privilege and the pleasure of perusing in the first instance the papers which are sent in to be read before the Institute; and you can well understand that the character of the first paper to be read before a new society—and more especially a society like ours which takes up subjects about which men are naturally so sensitive and so apt to become excited—must needs have been a matter of extreme anxiety to all charged with any responsibility in connection therewith. Well, upon reading Mr. Warington's paper, I feared—very naturally perhaps, but, as it turned out, very needlessly—what might be its effect upon “weaker brethren.” I also confess that I thought Mr. Warington had been over severe upon what persons out of the Institute would call *our own side* of the question; and that some might even conclude upon reading his paper—to use a phraseology we have been recently accustomed to elsewhere—that we, like the unfortunate engineer, “had been hoist with our own petard.” But a little reflection cleared away these apprehensions. Besides, there was but one course open to us, and that was, not to shrink from difficulties, if there were difficulties, but to meet them. In controversy and intellectual strifes, as in material wars, there is but one path that is tolerable to honest hearts—I believe it also to be the path of safety—and that is the path of honour. In the Victoria Institute I trust that shall ever be our path, come weal come woe. If there was matter, then, in Mr. Warington's paper to startle us, or to make us feel uneasy, that, in truth, was the very best reason that could be urged why such a paper should be read in this Institute. It at once—had we been inclined to forget it—reminded us of the serious issues that had been raised in the name of Science against the Holy Scriptures. It braced us to our work, and put us upon our mettle. Nay, our enemies themselves being judges, neither the ability nor the impartiality of Mr. Warington's paper can be questioned. I have ventured to make these remarks upon the present occasion, as upon a few incidental issues involved, I happened to differ from Mr. Warington's view, as may be seen from the discussion upon his paper. But, what was the result when it was read? There was at first a natural feeling on the part of some (such as that which I had myself experienced on first reading the paper), that it was unsatisfactory to have, as it were, such a bill of indictment drawn up against the Scriptures, concentrating all that had been said against them in the name of Science, without complete answers being given to the

various counts in the indictment. But I appeal to the report of the discussion upon the paper, and to the various answers which were then and there given to several of the principal objections to the Scriptures to which Mr. Warington had called attention, as the best proof that the Scriptures have nothing to fear from the strictest investigation as regards all such conflicting issues, and as the complete justification for opening our public discussions with the reading of Mr. Warington's paper, and as further proving the importance and usefulness of this society.

But I must now pass on to make a few remarks respecting the second paper read before the Institute. And here I have to remind you of the first loss we have sustained by the death of the author of that paper, our Vice-President, Dr. Charles Mountford Burnett. Our society is yet too young to require a formal obituary of its distinguished members who have passed away. But, young as it is, even in the first year of its existence, we have to lament the loss of a Vice-President and of one other member. But although Dr. Burnett and Mr. John Vanner appear as "deceased" in our Foundation List of Members, I have the satisfaction to be able to add that their names are not lost to the Institute, but are perpetuated in the names of their sons.*

The late Dr. Burnett's paper, *On the Difference in Scope between the Scriptures and Science*, is one to which justice has not been done by its critics, and which I therefore wish especially to notice here. To those who have read our *Journal of Transactions* it must be known that Dr. Burnett's paper was written while he was suffering from the illness of which he died. It had not therefore the benefit of any final revision by its author while passing through the press; and a sentence in it here and there may consequently be found not as definitely and clearly expressed as in other circumstances it no doubt would have been. To all courteous readers and all kindly critics, I am very sure, not another word need be said on this account; but in truth the arguments and main purport of the paper are sufficiently obvious to all who will read his words in charity, and construe them with reference to the context. The drift of the paper is as important as it is obvious. It draws attention to the fact that the Scriptures purport to tell us some things concerning nature which no natural science and

* Since this was written I have heard with extreme regret of the loss of another of our members, my friend Mr. Evan Hopkins, to whose papers read before the Society I hereafter refer. He died on Friday last.

no human investigation could ever reach. He especially reminded us that the final cause of death—that is, the reason why death has come into the world and seemingly invaded the Creator's work—could never be possibly arrived at by mere natural science; but that revelation does profess to account for it. But he might have gone some steps beyond this, both backwards and forwards, and told us that neither as to the origin of things visible, animate or inanimate, nor as regards the end, and what is to be when life is past, can natural science teach us aught. Nay, he might, as a natural philosopher, have used the pregnant words of Sir Charles Lyell, in his address as President of the British Association at Bath, in 1864, and said, "I will not venture on speculations respecting 'the signs of a beginning,' or 'prospects of an end' of our terrestrial system,—that wide ocean of scientific conjecture on which so many theorists before my time have suffered shipwreck,"—inasmuch as such speculations go beyond the scope of mere natural science. And yet we know, as thinking men, that we cannot rest satisfied with a knowledge merely of the present. I may cite another scientific witness to prove this. According to Professor Huxley, "The question of questions for mankind—the problem which underlies all others, and is more deeply interesting than any other, is the ascertainment of the place which man occupies in nature, and of his relation to the universe of things. Whence our race has come; what are the limits of our power over nature, and of nature's power over us; to what goal we are tending; are the problems which present themselves anew, and with undiminished interest, to every man born in the world."

But may we not ask, How could there possibly have been a "beginning of our terrestrial system" at all, without the Great First Cause, Himself without beginning? or, How can we conceive of what we call "nature," or "the universe of things," without presupposing the prior existence of the God of nature—the Omnipresent Deity? Then where do we find the answer to these inquiries clearly enunciated except in the Holy Scriptures? Natural science on these questions is acknowledged to be dark or dumb. And, if we take any other book known in the world, where else have we such a revelation, either of the beginning of things, or of our own present state, or of the end that shall be hereafter, as in the Bible? *It* gives the answer to "the question of questions for mankind;" *it* solves the great "problem which underlies all others." *It* tells us "whence our race has come;" *it* tells us "the place man occupies in nature;" *it* tells us "the limits of our power over

nature, and of nature's power over us ;" also, "to what goal we are tending." It reveals that "In the beginning God created the heavens and the earth ;" that "God created man in His own image," and made him "upright ;" but that man has sinned, and thus—in our great poet's words—

"Brought death into the world, and all our woe."

But it tells us more. It tells us of other powers than those of nature ; it tells us that though we are born in such a condition that "nature," alas ! has only too much power over us, yet we are so born with the hope of restoration and of rising superior to this power of nature. It tells us, in short, of the power of grace upon man's soul, and it points us to the goal of Heaven ! This revelation of our faith as Christians surely speaketh better things "to every man born into the world" than those comfortless speculations of mere human theorists, which hitherto have only led to shipwreck upon the wide ocean of scientific conjecture. Is this revelation, then, not something to be cherished ? Is it not something that may well unite together, as in this Society, all who profess and call themselves Christians, and whatever may be their minor divisions or differences, to make common cause in order to guard it with heart and soul against the common foe ?

But it may be said, this is pleading the cause of faith rather than of science. That is true. But, then, it is pleading for faith where science fails ; and, moreover, it is for faith based not only upon revelation, but upon all that the highest philosophy can teach us of things visible, and all that our own hearts can rationally conceive : it comprehends the highest science of the world and the highest science of man. True, the Bible reveals that "in the beginning God created the heavens and the earth ;" but is not that, also, the inevitable and rational conclusion of the Natural Philosopher ? Here true science and religion unite. This is the rational deduction of human reason and philosophy, though it is also an article of Faith. So, the Bible reveals to us the fall of man, and tells us of the consequent sin and misery. But then, man's experience in this responds sadly to revelation. We know too well both of the sin and of the misery ; the truth is revealed in our consciences and is witnessed in all we see among our fellow-men around us. The cause of all this, as revealed in Scripture, we believe, for we find no other key to the enigma of our life. The Gospel message thus "commends itself to every man's conscience," even before we think of going into proofs or evidences of the truth of Christianity. Study nature all around, we may say, as deeply and as thoroughly

as you will—for hath not God “set the world in man’s heart,” though “he cannot find out the work that God maketh from the beginning to the end”?—and nature will lead you up to nature’s God. “Know thyself,” we may also say—the more deeply and completely the better—and you will learn thy sin and thy need of a Saviour. When science, as is acknowledged, only flounders out of its depth when it speculates as to the beginning or the end of things; and yet, when it is confessed that the question of questions for mankind, and the great problem of human interest that underlies all others, is to discover whence our race has come and to what goal we are tending,—Is it wise to turn from the only source which gives us a complete and rational answer to these inquiries? And when Scripture thus goes beyond the scope of all that human science can teach us,—is it philosophical to disregard this marked difference between the scope of Divine revelation and that of human science? It was to this difference our late Vice-President desired to call our special attention at the outset of our career as a philosophical society, and, as it turned out, at the close of his own earthly course. Let us remember and revere this parting admonition. Let us also draw an illustration of its wisdom and practical application from another source than our own Transactions. In one of our early papers *The Various Theories of Man’s Origin* were discussed, and among them the Darwinian theory that derives man from the ape. Most persons, you are well aware, are unable to reconcile that notion with what is revealed in Scripture as to man’s creation. There are some others, however, you also know, who think they see their way to reconcile Darwinism with the Scriptures. The discussion of this question, and with the same issues, has been taken up in various quarters. Professor Balfour, of Edinburgh, has recently given an account of an episode arising out of one such discussion at a meeting of the British Association. The learned Professor, when in company with Sir David Brewster, was accosted by a distinguished Darwinian convert, and asked why he had said that the Darwinists did not believe the Bible? The Professor frankly replied, “The Scriptures teach us that man was made in God’s image; you say that he rose from a brute to be a man, and a man of the lowest grade, instead of being originally the perfect man the Bible tells us he was created, and from which original state of perfection he has fallen.” In answer to this the disciple of Mr. Darwin asked with unguarded candour, “How could a perfect man fall?” Professor Balfour did not reply directly to that interrogation. He did better. He proved from it he was right when he had said that Darwinism is irreconcilable with Scripture. He re-

torted, "You have proved that you do not believe the Bible ; for have you not just denied the fall of man ?" The Darwinian then was silent.

I would earnestly commend such considerations as this to any who, looking only at a part of the large issues involved, are inclined to attempt to reconcile the Darwinian theory applied to man with the Scriptural account of the creation. Take its whole scope, or, if I may say so reverently, the theory of man's creation (made "a little lower than the angels,") and of his present condition, as revealed in the Bible ; and even if ingenuity may be able to reconcile the brief record of his mere "creation" with some process of gradual development, yet surely what the Scriptures teach us as a whole respecting man is plainly and utterly at issue with the notion of man having risen from being an inferior and unintelligent or irrational creature to his present condition.

If we believe the Scriptures as a whole, it will not do merely to take the letter of some few verses in *Genesis*, when we discuss the question whether Darwinism is reconcilable with Scripture. We are bound as Christians, and as believers in Revelation, to consider the whole scope of the Christian faith as well as the full scope of what else is put before us by our fellow men as claiming our assent. But I am not in the least asserting that we may not also take lower grounds and join issue with Darwinism and all other human theories by arguments based upon our knowledge of nature. To do this, in fact, is one of the objects for which this Society has been especially established ; and it is thus, it will be seen by reference to our *Journal of Transactions*, we have striven generally to discuss the subjects we have had under consideration. But we must not be debarred from also reasoning upon higher considerations, nor frightened from the conception of faith, especially where positive knowledge and human science fail. We must remember, also, there are questions which human science only leaves in doubt, and as it were nearly balanced, as regards the evidence or arguments for this or that, or as regards the authorities for one opinion or another. It would be hard to say, for instance, whether the greater number of the most eminent Ethnologists or Anthropologists have come to the conclusion that man is descended from one or many Adams. In proof of this, I may quote from the last address of the late President of the Anthropological Society of London. Dr. Hunt says :—

"A French anthropologist not long since asked the question, whether the majority of the Society were in favour of the monogenist or the polygenist theories of the origin of mankind ? The reply I gave him was, that the

majority would be in favour of whichever theory should eventually appear to be true, and that at present they suspended their judgment, and did not give any preference to the various theories of man's origin."

Dr. Hunt frankly says this, though individually he considers, "under all the circumstances, that the polygenist theory is the most reasonable," while at the same time he also admits "it is an assumption of no great scientific value." Now, surely in such cases, when "science" gives no certain sound, if we have any rational grounds for accepting the Scriptures at all, and for holding the Christian faith, it is impossible but that what the Scriptures seem most obviously to teach as regards man's origin and history, and which has been the universal tradition among those to whom were first committed the oracles of God, must be received without doubt or hesitation.

The precise position for which I am now contending was well brought out in the third paper read before the Institute, *On Comparative Philology*, by the Rev. Dr. Robinson Thornton. Scripture not only teaches us of the creation of the world and of man's origin, but tells us of the origin also of that marvellous human faculty and instrument of reason, language or speech; or rather it narrates the origin of that variety of languages which we find throughout the world. Now, the investigation of this subject is a perfectly legitimate matter for rational inquiry. It is almost impossible, indeed, for thinking men to refrain from speculating on the subject. The variety of tongues among mankind is perfectly analogous to the varieties we find in the world as regards man's mental and physical development. We cannot help wondering how the present state of things could have come about. We see slight variations in outward appearance, in mental capacity, and in manner of speech, even in every family and village and city. We find greater differences still, and more marked characteristics, in different races and nationalities and countries. We see the work of variation and divergence on a small scale, within certain limits, going on before our eyes. We reflect, and look back and around us, and we discover that we are face to face with a great and intricate problem. And what do we find? We find that the most eminent philologist of our day, the accomplished Professor Max Müller, has come to the conclusion, from inductive reasoning upon the internal evidence furnished by analyzing the roots and structure of all the various languages of the world, that they must have had one common origin. Now, comparative philology, remember, is quite a modern science. How marvellous, then, to say the least, it is

also to find that this deduction of modern scientific inquiry agrees with and is best explained by the simple account of the confusion of tongues at Babel, written in the ancient Jewish Scriptures! There we are told that the earth was once "of one language and of one speech." And now it is found that when the roots and inflections of words in each language are traced back and back, we can perceive the common radical points from which the various dialects have diverged. Now, this might seem to settle the question upon its merits. Dr. Thornton, however, put it before us more moderately and modestly. Without claiming to have absolutely proved so much, he argued that if we find that the traces of common resemblance among the roots of the languages of the earth are sufficient to show their probable or even possible derivation from one common stock, then that alone ought to determine us in favour of the Scriptural account. And why? Because the Bible has other claims upon our consideration. We must take its whole scope, also the whole evidences in its favour proving it to be the revelation of God to man; and then, as regards this comparatively incidental episode narrated in the course of its grand history of the world and of mankind, we cannot but receive its teaching with reverence and faith, when we find there is nothing in science to show that "the very letter of the written word as we have it," as regards the origin of human dialects, is "untenable."

I now turn naturally from Dr. Thornton's valuable paper to that of Professor Young *On the Language of Gesticulation*, read at our first ordinary meeting this session. The Professor passed from the consideration of the highest to the lowest mode of human intercourse, from the expression of thought by articulate language to its expression by dumb signs and gestures. As no race of mankind has been discovered who have not a language, some persons might be apt to conclude that Professor Young's paper dealt unnecessarily with a mere imaginary condition of things that never had existence upon earth, so far as man's knowledge, and therefore so far as man's science, can extend. But we must remember that the learned Professor wrote his interesting memoir expressly to meet the imaginary speculations of those who, contrary to all this knowledge and science of mankind, are endeavouring with persistent zeal to teach the world, that man has probably been derived from the ape, and was therefore originally without speech. It is also actually the fact that for several evenings during the present session the Anthropological Society has been engaged in gravely discussing a theory of one of its

leading members, who, as a philologist, has arrived at the conclusion that the original inhabitants of Europe must have been *mutes*!

Having referred to this theory, I feel bound to give you some extracts from the paper in which it is put forth, that this allusion to it may not give rise to misunderstanding; for I find that the author of the paper, the Reverend Dunbar Heath, considers that mute men may be men who speak! He says: "I wish my readers to understand that by mute men I mean men who may or may not use words, but who only express emotions by them, and that such emotions are the *individual emotions* of the mute being." This, he concludes, was once the condition of the inhabitants of Europe, "in the days of woolly elephants and rhinoceroses," and of kitchen-middens. But he wishes it "to be clearly understood that he is not writing and has never yet written on the origin of language, but on the transmission of language from tribe to tribe when once it has been acquired."

He then curiously proceeds to give his readers a graphic conception of the state of things among Europeans before the Aryans came among them to teach them how to speak. He says:—

"I am about to bring before the reader a conception of certain kitchen-middens occupied by what I call mutes, and subjected to the rationalising influences of a further advanced set of men whom I call speakers."

In explanation of this theory he "divides the development of nature, between the nebular chaos and the present state of things, into the three most fundamental of all possible groups or divisions." The first comprises "the organization of matter;" the second "those [*sic*] which bring these early organisms into a sensationalised or emotionalized state;" and third, "the rationalising of emotions." He adds:—

"That vast time was taken in the organizing of matter, I take as a proven fact from the hands of geology and physical cosmogony."—"I mean by this, that there was a vast time during which our mundane system contained matter without organism, and a further vast time during which organism was increasing in complexity before it arrived at its present state."—"That there was also a vast time during which organism existed without sensation, is equally admitted. Vast is the step from unorganised matter to organism, but equally vast is that from organism to sensation."

You, in this Institute, as rational beings, will no doubt readily admit that "vast is the step from unorganized matter to organism," and "from organism to sensation." To us it will appear analogous to and quite as vast as is the difference

between the undug, unformed clay, or the unbuilt bricks and mortar, and the stately building constructed in beauty, with art and skill, by an architect's intelligence, or between an organized but inanimate body and a living creature. We may also *here* be incapable of understanding how any amount of mere time could possibly bring about such differences. Mr. Heath seems himself to perceive this, and yet to shut his eyes to the "evidence" he adduces. For he goes on :—

"If the lowest organism is that which in the sunlight can simply decompose the inorganic carbonic acid, and appropriate to itself the carbon, there is no evidence of sensation accompanying such an act ; nor have we in the whole phenomena of vegetable life any reason for supposing sensation. If, again, the lowest animal organism is that which can decompose the lowest vegetable organism, and join to itself its organic elements, there still appears in this no evidence of sensation."

And what does he therefore conclude ? He says :—

"We admit, therefore, there was a vast period before evidence of sensation appeared, and that sensation has increased during vast periods in nature since its first appearance."

So much for Mr. Heath's theory in the abstract. Now let us quote his illustrations of how the rationalising of sensation or emotion might have been produced. He says :—

"That I may direct the thoughts of my readers into a channel which would lead I think to profitable results, I will give two instances of what I mean by rationalisation of emotion. One shall be in low mute mammal, the other in speaking man.

"I will suppose, then, a low mute mammal, such as a tame guinea-pig, to be taken by the inexorable boy who owns it for a series of nice warm-water baths, on the plea that it is good for the wretched animal to have a washing. Now, judging from what I hear has happened in such a case, I understand that for the first time or two the animal shows mere abject emotion, by its utterance of piteous cries or squeals. At the third, or even second time, however, the emotion diminishes. The rationalised emotion becomes an idea of a something external to be resisted. Ratio or comparison begins to take place. The cry now becomes a grunt of anger, which is a less absorbing emotion than fear ; and, like General Lee, the overpowered animal at the fourth time fights, bites and scratches to its very utmost, deserving a better fate." (*Sic*)

I fear you will consider I am open to some censure for not having furnished Dr. Thornton with such a specimen as this of the *Logic of Scepticism*, that he might have put the steps of this "process of rationalising the emotions of a guinea-pig" into syllogistic form, in the excellent and only too brief paper which he read at our last Ordinary Meeting. But the fact is,

I only came upon this last Saturday. It is amusing to read such arguments; but I agree with their author that it is "profitable" also. Not in his sense, perhaps, but still it is profitable for us to know how the process of "rationalising" is going on among our fellow men, and in other scientific societies. But I must now give you Mr. Heath's other illustration. He proceeds:—

"In the next example rationalisation has advanced still further. Let the mute mammal be a speaking man, walking along a lonely road, and the inexorable boy a highway robber. A sensible or rationalised man, when met in such a case, feels neither fear, nor even anger. He recognises the inevitable, hands out at once his purse, and politely hopes the wife and family of the depredator are salubrious. Here the power of language enables the two parties so fully to understand each other, that the natural inward individual emotion passes wholly into an external interchange of ideas common to the two."

The guinea-pig's emotions, you may remember, became rationalised by its being four times washed, and so made angry, and to bite and scratch; while the man *is* rationalised, because at once he meekly submits to be robbed! * It might be profitable—I am sure it would be amusing, though also somewhat sad—could I go on with still larger extracts from this anthropological paper, *On the Acquirement of Language by Mutes*. But I must be brief. I am not surprised to find Dr. Hunt thus expressing himself this year in his Annual Address to the Fellows of the Anthropological Society: "It

* When revising the proof-sheets hereof, I was unexpectedly furnished with a denial of the accuracy of the small experiment relied upon by Mr. Heath. Two of my boys being present, I read aloud the account of the result of bathing a guinea-pig, thinking it would amuse them; when, greatly to my surprise, the elder boy interrupted me with the exclamation, "That is not correct!" I had quite forgotten at the moment that the boys had themselves kept guinea-pigs, and might possibly have made the same experiment. This, they told me, they had done frequently; but with no experience corresponding to that of Mr. Heath's "inexorable boy." Their guinea-pigs, when taken to the bath, never fought, bit, squealed, or scratched; but, on the contrary, they took to the water kindly—nay, "sometimes they jumped in," and "seemed to like it." These guinea-pigs, in fact, were quite *un*-"like General Lee," but rather resembled the "rationalised" simpleton who is supposed to exchange compliments with the highway robber who attacks him, instead of knocking him down. But this result, though contrary to that upon which Mr. Heath has founded his reasoning, may answer his argument quite as well, and will harmonize his two examples. Perhaps he may now be satisfied that even a guinea-pig's emotions may be "rationalised" beyond the fighting and biting point! To jump voluntarily into cold water and swim about must surely indicate a step in rationalising beyond the mere feeling that to be plunged into a "nice warm-water bath" is an "idea of something external to be resisted"!

no doubt (he says) often occurs to those who attend our meetings, or read reports of the same, that they do little towards the establishment of a science of mankind." He adds: "This feeling is no doubt greatly based on truth"; and this frank admission disarms criticism, and is well borne out by the specimens I have given of what is contained in last month's number of the *Journal of the Anthropological Society of London*.

Mr. Heath further says:—

"The scientific evidence in favour of the traditionary view [*i.e.*, the account in Genesis of man's creation] being absolutely none at all of any kind whatever, I compare it therefore unfavourably with the other view now rising into public notice. This view is, that during and after the tertiary geological epoch, the highest mammals then on earth were becoming more erect in their way of walking, less hairy in their bodies, and more like in general to what the lowest men are now. Such beings are supposed during these changes to have also gradually rationalised some of their emotions, by the use of mental powers, [but] not so much beyond what the average of them possessed as to presuppose a miraculous development."

Then he says:—

"If we can by this time conceive to ourselves the clever chief of two or three hundred of such merely emotional inhabitants of a kitchen-midden, struggling into the semi-emotional, semi-rational state of expressing, 'I *will* kill,' we shall now be able all the more readily to follow such a chief, and his tribe, in the circumstances under which I proceed to depict them."

Then he depicts some half-dozen well-armed and speaking Aryans coming suddenly upon two hundred of these European "kitchen-middeners," and imposing their language on the mutes, which they (he thinks) would at once adopt, only modified by Grimm's law. He goes on:—

"Now follow the leader of the six Aryans in his first lesson to the crowd of 200 mutes around him."—"Naturally he would get the crowd to pronounce after him some short syllables such as *pa*, *ta*, *ka*, to illustrate the use of lips, palate and throat, and very naturally the four or five men (or women more likely), just in front of him would pronounce them rightly, but not one man in fifty can tell the real effect of his work on a crowd. On returning to their wigwams much would be the emotion of risibility and imitiveness displayed that night among the natives;" &c.

But I perceive that "the emotion of risibility" is here becoming so considerable, that I shall not attempt to quote consecutively what follows. The Aryan leader is supposed to find his pupils extremely frolicsome and refractory, and rather enjoying their apt capacity for mispronouncing every

letter. And upon the whole, some of them at least prefer "the wrong utterance of *ba, da, ga*, instead of the original *pa, ta, ka*." But still, "the best thing the leaders could do when their teacher tried to show them their error of tongue-force would, no doubt, be done by them. They would screw their faces amidst shouts of laughter into the nearest approach they could manage into what was right, but as to correcting yesterday's error, once irretrievably made, even if they did so themselves it would be too late." Nay, Grimm's law would still prevail, "and *ba, da, ga*, would become *fa, cha, tha*." "I can hardly conceive," in conclusion, says Mr. Heath, "Grimm's law to have arisen except at once, in a day, at a stroke."—"Let some better theory than my own be propounded. At present there seems none other which professes to account for Grimm's law."

I think, after hearing this, you will turn with some relief and with fresh zest to Professor Young's paper in our own *Journal of Transactions*, as somewhat more profitable reading.

But before Professor Young read his paper, we were favoured at our opening meeting with an introductory Address by our Vice-President and Chairman, upon a graver subject, *The Doctrine of Continuity*, as enunciated by Mr. Grove, the President of the British Association, last year, at Nottingham. This was not, however, the first time that Mr. Grove had put forward the same views, although upon that occasion he especially identified them with the theory of Mr. Darwin, which he had not done before. The following extract from Mr. Grove's well-known work, *On the Correlation of Physical Forces*, will show you to what conclusions the doctrine of continuity also leads as regards the universe. Mr. Grove says:—

"The views of Mr. Thompson differ from those of Laplace recently enforced by M. Babinet, which suppose the planets to have been formed by a gradual condensation of nebulous matter. A modification of this view might, perhaps, be suggested, viz., that worlds or systems, instead of being created as wholes at definite periods, are gradually changing by atmospheric additions or subtractions, or by accretions or diminutions, arising from nebulous substance or from meteoric bodies, so that no star or planet could at any time be said to be created, or destroyed, or to be in a state of absolute stability, but that some may be increasing, others dwindling away, and so throughout the universe, in the past as in the future." (4th ed. p. 104.)

Here we have the theory of "the self-evolving powers of nature" put plainly forth—the "doctrine of continuity," fully stated. The sum and substance of it confessedly is, "that no star or planet could at any time be said to be created;"

and this is surely in direct opposition to the words we accept as revealed truth: "In the beginning God created the heavens and the earth." Now, let me ask this question, bearing upon the public enunciation of such views of nature: In what learned or scientific society in this great metropolis, excepting our own, could or would this subject be discussed? And remember that although this speculative theory was put forward in the British Association, it was spoken from the presidential chair, at a general meeting, where no discussion, of course, was possible, or intended. Moreover, there is no "philosophical section" in the British Association, and none in which a paper upon such a subject would be allowed to be read, or in which it could be discussed. I think it therefore to be regretted, and even deprecated, that any such speculation should have been enunciated in an address from the presidential chair at our great scientific congress. It was not quite fair. It is that kind of thing which unhappily brings science into disrepute. It was little else than preaching "the doctrine of continuity," *ex cathedrâ*, without permitting any right of reply.

Mr. Mitchell well described this theory as being founded upon an evident unreasoning dislike of all that is miraculous in nature, or reminds us of the hand of God and of special Providence. That, also, is a subject which no other society in London but our own can take up and discuss; and we have already discussed it. The Rev. W. W. English has contributed to our proceedings a most able paper, *On Miracles, their Compatibility with Philosophical Principles*, which I am certain was heard or has been read by all our members with the greatest satisfaction. Mr. Penny's *Thoughts on Miracles*, read the same evening, gave us what might be called the extreme of the anti-sceptical view; and the two papers, taken together, served as an admirable basis for the discussion that followed. But, completely as the subject was handled in these papers and in the discussion upon them, it is not entirely exhausted. The important volume put forth recently on *The Reign of Law*, by the Duke of Argyll—one of the most important philosophical works that has been published for years—reopens this question, and is likely to call forth some rejoinder, and to bring the matter again before the Institute. In the past discussion here there was no essential difference of opinion as regards principles, though we managed to differ in our mode of viewing certain things and in our mode of expression. I might almost say the terminology of the subject was proved to be unsettled. In Mr. English's reply he said—

"Nature, if it includes Deity, (and I see not how it can *exclude* it,) comprises all that is *possible* as well as actual."—"Those 'higher laws' I referred

to, are *moral* and not physical: those principles, in short, according to which all things are wisely governed. Miracles may be real or apparent infractions of material sequence, but they are, nevertheless, fulfilments of 'higher laws' of moral government."

Now, agreeing entirely with the spirit and full meaning of these passages, I yet beg leave to observe that I doubt whether their phraseology will be accepted by the sceptic, or can be regarded as strictly accurate. *If* nature is considered as including Deity; and *if* moral as well as physical "laws" are accepted as ruling in nature, there would scarcely be a difference of opinion among men as to miracles. But unfortunately those who have denied the possibility of miracles do generally deny God; and with them "the laws of nature" only mean the invariable material sequences, or "laws" so-called, which they think they can imagine not only to operate but to exist without reference to Deity. Now, it is here that a confusion of thought intervenes, which may puzzle the honest thinker, and which may temporarily subserve the arguments of the sceptic or atheist. Let us be frank upon such high subjects, and endeavour to understand the mode of thought that influences others, as well as our own conceptions. Although, then, in the works of nature we may see the hand of God, and the deeper we study the more clearly may we recognize His nearness, and that "in Him we live and move and have our being;" yet, let us also remember that in nature God is veiled: "Who by searching can find out God? He passeth by me, and I see Him not." We conclude, reasoning from what we see, that there must be a God, a Great First Cause, but still a Cause *Invisible*. We see His operations around and about us, but we cannot trace Him more definitely: "His path is in the great deep; but His footsteps are not known!" We do not by nature know God; we only, as it were, "feel after Him:" "Such knowledge is too excellent for us, we cannot attain unto it." But His laws—"the laws of nature"—we can see, and in a measure learn to know. And it is indeed such knowledge that properly constitutes natural science. Now these laws, it cannot be denied, are in many instances, so far as we know them, *invariable—or they seem to be so*. I make this qualification for the best of reasons. I know no law of nature so evident or apparently so invariable as that of terrestrial gravitation, and yet our most distinguished chemical philosopher, Professor Faraday, in his treatise on *The Conservation of Force*, thus expresses himself:—"The usual definition of gravity as an attractive force between the particles of matter

varying inversely as the square of the distance, whilst it stands as a full definition of the power, is inconsistent with the principle of the conservation of force." Then as regards our actual knowledge or experience of this law of gravity, when we test it at the surface of the earth, do we find it even then invariable? On the contrary, we know very well that there is a marked variation in the weight of bodies—*i. e.*, in the force of their gravity—at the equator and in arctic latitudes; and this is equally inconsistent with the law of "the conservation of force," as is the variation arising from distance in space. Or, grant that these variations are practically the same, which I need not dispute—but I am obliged on the present occasion to touch lightly on such illustrations—still I wish to point out, that if both theory and our actual experience affirm that the force of gravitation *varies*, then the law is not "invariable"; and, at any rate, according to Professor Faraday—and as must almost be self-evident—a force that thus varies must be inconsistent with the conservation of force. It is also inconsistent with the law of *continuity*; for all who have studied modern physical astronomy know that the force of gravitation theoretically grows weaker and weaker, till it dies away, and that bodies are supposed to get out of the sphere of their former centres of attraction, and to travel away into space. Here, to use our Vice-President's apt expression applied to another illustration, "the law of continuity snaps asunder," and indeed Mr. Grove himself has told us of the "dwindling away" of his imagined worlds.

I need not here enter upon the refinement which might be advanced, that a "law" which defines that a certain force in nature varies invariably, and varies in a given manner, may be truly regarded as an invariable law. I will only remark, this is very like a law of continuity which will neither admit that worlds have been created nor that they can be destroyed, nor yet admit them at any time "to be in a state of absolute stability"! but tells us that they "dwindle away," "ever changing, ever new," the very reverse of all our ordinary ideas of continuity!

But to resume. The uniformity, real or apparent, of nature's laws, such as that of gravitation, or those of chemical combination, of light, of heat, of vegetation, or of life, so far as we think we understand them, all seem to speak to us of certain principles at work, energizing, as it were, of themselves, according to some rules or laws which constitute what we call their nature; and upon the whole we may admit that these laws do act uniformly and invariably. There is, then, this settled course of things, which we call nature, and which

some piously ascribe to God's will, but which others regard as merely "the nature of things" themselves, without any reference to Deity. Those who hold this latter opinion—certainly those who have ventured so to express themselves—are a small and insignificant number. They, of course, cannot possibly believe in any interference with these laws of nature. Their idea of law does not include a law-giver. According to them it is material and unintelligent nature that produces all we see around us; and with them a miracle is impossible. But there is the other class who believe in God as the author of nature; and of those, some have got the idea that this nature is absolutely fixed, and they have the notion of uniformity and invariability so settled in their minds, that they cannot imagine the Deity to interfere with the laws of nature He has once established. They also, therefore, deny miraculous interposition, although they are not professedly atheists. It is to meet the difficulties of this class of thinkers, that some have recently endeavoured to show that miracles may be regarded as possible, without supposing that there is any infringement of nature's laws, if we merely suppose that there are probably another series of higher laws that occasionally come into operation, and produce miraculous effects. But it is here we come upon the issues raised in Mr. English's *Reply*. I am not now about to discuss these issues. I will only ask: Can this reasoning satisfy this class of objectors to miracles? And is "law," in point of fact, used in the same sense in the two cases? Is the invariable or ordinary law, a law in the same sense as the occasional and exceptional "law" that intervenes? If the latter is admitted to be "*different from*" the settled course or ordinary law, is not that enough, without insisting that what is different from what is settled, being *unsettled*, is therefore "the contrary"? But there is another question I must ask: What is gained after all, if we get it admitted that miracles are the operation of some higher law? Is this law self-acting? Does the universe go like clock-work; and do miracles, as it were, strike at appointed hours as the hand of time goes round? If not; if the so-called "higher laws" are the laws of moral government, and miracles are interpositions by the Creator, Himself above all law and without law,—then are not miracles best described as simply wrought "by the finger of God"? Is the "law" that works them aught but His will? And, after all, the practical question is—without discussing the accuracy of the phraseology:—Will those who deny miracles because they think that nature's laws cannot be infringed, accept this view, that miracles result from the operation of other "laws"; and, if not, of what

value is the argument? For, if common or ordinary things, and miraculous or wonderful things, are all equally supposed to be done according to laws, will not the denier of miracles merely also deny, and *à fortiori*, the existence of the laws to which the miracles are attributed?

For my own part, I venture to think there is another remedy required for this state of mind—another line of argument which should be adopted; and that I expect we shall have advanced in the paper to be read at the close of this Session by Professor Kirk, *On the Relations of Metaphysical and Physical Science to the Christian Doctrine of Prayer*. This also is a subject, you are aware, upon which the challenge has recently been thrown down, in the name of science, to all who believe that God hears and answers prayer, by an eminent scientific man, and upon the same specific ground of the uniformity and invariableness of the physical laws of nature. This again, let me remark, is a subject which could not be taken up in any other scientific society in London except our own. I observe from an advertisement this morning in the newspapers, that an essay by Professor Tyndall, *On Miracles and Special Providence*, will appear in the June number of the *Fortnightly Review*. We should have been glad to have read it here;—and here, I need not say, it would have been discussed more freely than it is likely to be in the press.

It would not be desirable, however, nor wise, to have our minds always as it were upon the stretch, and engaged exclusively in the contemplation of such high subjects. Nor, as you are aware, have objectors to Scripture always taken such high or abstract ground. Many of those objections most prominently advanced in our day, have been, we may almost say, based upon the minor details of geological science, which only require to be tested by reference to facts and by inductive proof of a scientific kind, without involving philosophical or metaphysical considerations. And so we have had several geological papers read before us. In Mr. Hopkins's two interesting memoirs, we had a theory brought before us which he had put forward many years ago, but which was contrary to then current views, and received little attention as a whole in scientific societies. Some of his views, however, were partially discussed from time to time, and some of them came to be established and accepted, in spite of previous neglect and opposition.* His main theory, that electro-magnetism is probably the great active cause of certain important geological

* *Vide, Journ. of Trans. of Vict. Inst., Vol. I., p. 33.*

operations, has been most ably and unexpectedly advocated as worthy of the most earnest consideration, in Professor Kirk's admirable Discourse, *On the Various Geological Theories Past and Present, and their Relations to the Teaching of Scripture*. But I must not now dwell upon this subject. I am sure Professor Kirk's discourse will be felt by all who read it to be only second, if not equal in importance, to his own most valuable work on the *Age of Man*, the merits of which have been so frankly acknowledged by Sir Charles Lyell.

In another paper on *The Lessons taught us by Geology*, by the Rev. James Brodie, we had a class of views put forward, some of which had been made the ground of sceptical arguments, but which he showed were quite compatible with the most confiding faith in revelation. On the same evening there was read another paper, by Dr. Gladstone, F.R.S., *On the Mutual Helpfulness of Theology and Natural Science*; in which he argued that revelation and science have not in fact been truly at issue, but that differences have rather been felt between mere human interpretations of the one or the other. The gist of these papers and of the discussion upon them was to establish fresh pleas for the reinstatement of natural philosophy as the proper hand-maid of religion—as Lord Bacon declared it to be—showing that she may fitly wait with all due deference upon “the Queen of Sciences,” Theology.

Our next paper was *On Falling Stars and Meteorites*, by our Vice-President, Mr. Mitchell, and it worthily completes No. 4 and the first volume of our *Journal of Transactions*. One would have imagined that a subject like that might have been considered as entirely out of the range of polemical interest. But unfortunately it is not so. Our knowledge of falling stars, so-called, and of the occasional fall of meteoric stones upon the earth, taken in connection with the discovery of the minor planets or planetoids, have stimulated scientific conjecture, and the imagined meteors which, in 1863, were given out by the President of the British Association, as the probable grand means for supplying the sun with fuel to prevent it from burning itself away, have since been imagined to be thrown out from the sun, and afterwards, upon coming into collision in space, to conglomerate into planets and to form worlds like our own! You have all heard of the Lucretian notion of material things having been formed by the “fortuitous concourse of atoms.” In the 19th century we have improved upon that, and imagine worlds to have been formed by the mere fortuitous concourse of meteors! M. Le Verrier, also, some years ago,

imagined there must be several rings of invisible meteoric bodies revolving between the sun and *Mercury*, and between the orbits of some of the other planets, in order to enable him to balance the solar system upon the calculations of theoretical astronomy. When the sun's distance, however, was recently reduced by four millions of miles, and the earth's whole mass reduced by one-tenth, the theoretical necessity for these imagined rings of meteors was no longer the same; but they seem to be such very great favourites at the present time, that the conception of their existence has been allowed to remain undisturbed. Even the few cold days we had a week ago have been publicly attributed to "probably the intervention of a ring of meteors coming between us and the sun," instead of to intervening clouds or fogs, or other ordinary meteorological causes. In Mr. Mitchell's paper, I need scarcely say, you will find no such extravagant theories advanced, but a very complete account of all that is known on the subject, with an honest and truly scientific confession of our real ignorance of the causes of all the various classes of meteoric phenomena.

In to-day's *Morning Post*, I find this account of a paper read by Professor A. Herschel before the Royal Institution, last Friday, on the same subject. He throws out a theory of his own as regards the origin of meteors—namely, that the zodiacal light is emitted from an immense number of small solid particles surrounding the sun in the form of an elongated spheroid, or double cone, and that the meteors are constructed from these particles. This theory has at least this advantage over others, of not having been hitherto proved to be fallacious; but, the report goes on to say, that the explanation he gave of it and of the formation of meteors was so misty and obscure as to throw little light on the causes of the phenomena which meteors present.—So, you see, we are again brought to our own Vice-President's conclusion on the subject, namely, that we are simply *ignorant*.

We have also devoted no less than three evenings to the discussion of the *Credibility of Darwinism*, which was advocated chivalrously by Mr. Warington, although not himself a convert to the truth of Mr. Darwin's views, with the intention of claiming for the theory a fair consideration, apart from prejudice, as a scientific hypothesis that may be entertained as *primâ facie* possible and credible. As I ventured to differ materially from Mr. Warington upon this subject, I shall say nothing here as regards this controversy, except that I think the discussion, when printed in our Journal, will show that

here there is every disposition to discuss either this, or any other theories which may have been adopted by our fellow men, as impartially and thoroughly upon their merits as the theorists themselves could desire. But I must add, that I think it will be expected, whenever a *bonâ fide* Darwinian is inclined to enter the lists, that some answer will be given to the arguments adduced against the theory in Mr. Mitchell's *Inaugural Address* last year, and in his remarks upon Mr. Warrington's Paper. I venture to say that there is nothing in the literature of Darwinism at all to equal, or which in scientific value will bear the least comparison with, Mr. Mitchell's arguments against the theory, especially as regards the formation of the eye and the cell of the hive-bee. I will here, also, further venture to say that I anticipate that his Paper on *Crystallography*, to be read on June the 3rd, will be the most valuable contribution hitherto made to that interesting and important branch of science. I regard it, indeed, as a special Providence with reference to the success of the Victoria Institute, that our Vice-President, Mr. Mitchell, has occupied this leading position in connection with it. I must apologize to him—not to you—for speaking thus of him in his presence. But it is my duty on the present occasion—and I must perform it—to direct your attention to all that has contributed to our success.

We have also had under consideration Mr. John Stuart Mill's theory of *Utilitarianism*, a philosophical subject which could only be fitly discussed in this Institute and in no other Society in the metropolis; for it seems to range beyond the scope of mere "Social Science" though it necessarily involves considerations bearing directly upon both social science and morals, as well as religion.

The last paper read in the Society was that by Dr. Robinson Thornton, already referred to, *On the Logic of Scepticism*. I have mentioned its one fault,—its brevity. But it is a paper to make men think; and it is also a good specimen of the kind of papers which are wanted in our day, to keep us from drifting in the direction which may happen to be the fashionable mode of scientific speculation. Should anything further be wanted, besides what the paper itself contains, or besides what I have said upon the present occasion, to show the necessity of directing attention to the "reasoning" or "logic" of some of the most advanced sceptics who are the especial and professed votaries of science, it may be supplied from the concluding passages of Dr. Louis Büchner's work on

Force and Matter, which went through no less than eight editions on the Continent before its English translation was presented to the British public by Mr. J. Frederick Collingwood, Fellow of the Anthropological Society of London. Dr. Büchner thus concludes his volume :—

“To those who may, by some of the results of our investigations, have felt themselves shaken in their philosophical or religious convictions, we recommend the following passage of Cotta, as a fit conclusion of this chapter, and of the whole work :—

“‘Empirical natural science has no other object than to find out the truth, be it, according to human notions, consolatory or the reverse, beautiful or ugly, logical or illogical, rational or absurd, necessary or contingent.’”

Surely when reasoning men, claiming to be natural philosophers, are not ashamed to avow that they are prepared to accept as truth, in natural science, what “according to human notions” may be illogical, irrational, and absurd—to say nothing of what is ugly or the reverse of consolatory—it is high time to call men’s attention to “the Logic of Scepticism.”

Having thus reviewed, although but imperfectly, the work in which we have been engaged during the first year of our existence as a Philosophical Society, it will probably be expected now, that I should say something of our future prospects ; but I fear further to trespass upon your patience. I will however say, that I am sure I shall but speak the common sentiment of every member of the Council and of every Member and Associate of the Institute, when I express our heartfelt thankfulness to God for the success that has hitherto crowned our labours. And, as our past has thus been blessed by Him from whom all blessings flow, so let us humbly pray and hope that our future also may and shall be blessed. At the same time let us remember that we have our part to do, that we are engaged in a noble work, indeed in a glorious warfare. We may not, therefore, listlessly sit down, and rest contented with what has been already achieved, but prepare ourselves diligently, so far as we are able, to enter upon fresh labours and to engage in fresh intellectual conflicts.

In conclusion, I ask once more, in words with which you are already familiar, “What nobler pursuit can man engage in, than in trying to discover Truth by the philosophic study of God’s works of creation ; and in what respect can Christians better employ themselves, than in thus discovering ever fresh proofs and confirmation of the revelations contained in Holy Scripture ?”

Rev. A. DE LA MARE.—Mr. Vice-President, I feel myself particularly privileged in being permitted to propose the Resolution that has been entrusted to me, namely, a vote of thanks to the author of the Address just delivered, together with a request that he will have it printed for circulation. (Hear, hear.) I consider that Mr. Reddie's address this evening is a most apt and becoming conclusion to our first year's proceedings ; and that the *résumé* he has given us of the papers which have been read before the Institute is most valuable in itself, and will be most gratefully received by those who have not had the opportunity of following our work so exactly in detail as he has kindly brought it before us this night. I may perhaps be permitted to express my own regret that illness has prevented me from enjoying a constant attendance here, but this has made me appreciate Mr. Reddie's address this evening all the more, because it has not only brought before me certain points, which I shall rejoice still more to re-consider when the papers are published, but he has suggested many things which perhaps, in casual reading, and when one has a large amount of work to perform, might escape one. I shall now, however, pass from the Address itself to consider the work of the Institute as it has been now brought before us : and I cannot help feeling that the most ardent expectations of any of us who were first in the field in joining this Institute have been fully borne out by the fruit which has been produced during the past year. (Hear, hear.) It does seem to me, not only that most important subjects have been most earnestly and most admirably treated, but that the result has been in one direction from beginning to end. The subjects have been treated as a Christian Institute ought to treat them ; and whilst I fully subscribe to Mr. Reddie's remark, that we are bound to receive and consider any paper written in a right, candid, and philosophical spirit, yet as an Institute of men who profess to be Christians, I think that all such papers ought to be discussed in a Christian spirit ; for this is not only an Institution differing from all other philosophical institutions on this very ground, but I think that upon this very ground it must commend itself to the judgment of all. With regard to the various subjects which have been under consideration, I think we seem to have touched upon all those points, or at any rate upon the most salient points, of the alleged difference between scientific results and the results of that queen of all sciences, theology. Certainly every individual, in viewing our work, may be considered to view it from his own stand-point, and I look at it in its connection with our theological studies. I should be sorry to advocate that theology should be in the slightest degree propped up or depend for its defence upon science ; but at the same time I should just as much regret that theology should as it were give the cold shoulder to science, and seem to thrust it aside as unworthy of consideration ; and just in the same way as I think we ought not, in the sphere of our Institute, to allow science to be thrust upon us simply because of the names it may bear and the quarter from whence it proceeds. (Hear, hear.) Let us act fairly with the one and with the other. We do not seek to advocate theology or to ward off facts, by appending a long catalogue of either the philosophical or intellectual

powers of those who have accepted it ; and so we do not feel disposed to be thrust aside in our theological pursuits upon mere scientific authority. We have often had certain results put before us, which in many instances have afterwards been obliged to be withdrawn, sometimes by the persons who put them forward, and who yet would thus have put us out of the field, and have condemned theology, by reason of the mere changing "science" of the day. I think, therefore, our Institute has been greatly useful in ventilating and canvassing the consideration of current theories ; and I feel convinced the further we go, and the more completely things become patent to us, the more we shall find that these two branches of the one great science of truth will be brought nearer and nearer together ; that the evils of rash and faulty interpretation of Scripture on the one hand, and of ill-digested theories about science on the other, will pass away, and then we shall fully realize that true science will ever be the faithful handmaid of true religion. I perhaps may be permitted, in conclusion, to add one sentence with respect to the gentleman who has delivered the address this evening. He has spoken—and I am sure the sentiment was expressed in the highest terms he could use, but not higher than was due—of our excellent Vice-President, who this night presides over us ; but whilst I would not be thought to infringe upon anything which Mr. Reddie has said in commendation of our Vice-President, I think our success is also greatly due to the exertions of our Honorary Secretary, who may be said to have been the first in the field, and who never seems to have flagged from that hour to this. In thanking him, therefore, for his excellent paper, I think we should connect with it our personal thanks for his exertions for this Institute, from its foundation to the present time. I beg, with your permission, to propose a vote of thanks to the author of the address, with a request that he may permit it to be printed and circulated.

WILLIAM H. INCE, Esq.—I beg cordially to second this vote of thanks and the proposition of the Rev. A. De la Mare, that this able and masterly paper of our Honorary Secretary be printed and circulated.

Professor MACDONALD.—Allow me to make one remark. I am delighted to hear the motion, in which I am very pleased to agree, for the printing of this address ; but I am going to ask our excellent Secretary, with reference to the very amusing sketch that he gave of another society, and of a paper brought before it by Mr. Dunbar Heath, whether it would not be more prudent (now that we have had the amusement of hearing this) not to print that part of the address. I think it would be desirable not to do so. There is another observation which I think you will excuse me for making, for I do it in a kindly feeling for the advancement of this Society. Mr. Reddie referred to a certain "small" party, and said he considered them as "small and contemptible"——

Mr. REDDIE.—I beg your pardon. I do not think that the word "contemptible" occurs in the paper ; "insignificant" is the word I think you mean to refer to.

Professor MACDONALD.—This is a Christian Society, and we should use

Christian expressions. Let us be clear and explicit upon everything ; but I thought it was rather a strong expression that was used.

The CHAIRMAN.—I am sure that all of you will most heartily join in the request which has been proposed by Mr. De la Mare and seconded by Mr. Ince, that the most excellent address of our Honorary Secretary should be printed. I am sure I congratulate myself very much upon the fact that the pressure of other work for the Society has caused this task to fall upon Mr. Reddie. It has given me a rich treat to hear what he has read ; and I am glad it fell to his lot to prepare it. I am sure it will add very much to the benefit of the Society ; and I may, at the same time, be perhaps permitted to say that I differ with my friend, Professor Macdonald, as to omitting any part of it—

Mr. REDDIE (to Professor Macdonald).—I have found the passage you referred to. The words I used are, “a small and *insignificant number*.”

The CHAIRMAN.—I think the discussion in the Anthropological Society, referred to by the honorary secretary, is a matter which I should object to have withdrawn from the address ; because such discussions show the necessity for our Society. These things go forth to the world unchallenged as “science ;” and we have a right to see whether they can bear the character which is put upon them by a professedly scientific society. Mr. Heath’s paper, also, was upon a kindred subject to one discussed here, “on the language of gesticulation” ; and I think the contrast between the manner in which the subject was treated in this Society, and brought forward in another professedly scientific society, is a fair subject for criticism. These are matters of interest of the day ; and I think we must openly meet them, or we should appear to be content with no reply to the absurdities brought forward under the guise of science. I beg to put the Resolution to the meeting, as proposed by Mr. De la Mare and seconded by Mr. Ince, that a vote of thanks be given to Mr. Reddie for his address, and that that address be printed separately and also in the Transactions of the Society. (Applause.) There is one thing I cannot sit down without alluding to ; that is, as proposed by Mr. De la Mare, that we add the thanks of the Society for Mr. Reddie’s indefatigable exertions on its behalf. No one but those associated with him, as I have been, can tell the vast amount of labour that has devolved upon him, not only in the first organization of the Society, and in his continual attendance at the meetings of the Council ; but also in considering the papers before they are read, and in editing them after they have been read, and revising the discussions that take place. No one but those associated with him, can form the slightest estimate of the amount of labour which he so cordially gives to this Society. I may say it is one in which he takes a vast interest, and he manifests that by devoting the most indefatigable labour to its account, and I think the best thanks of the Institute are due to him.

The resolution was carried unanimously.

Mr. REDDIE.—I beg leave to acknowledge the very kind way in which you have thanked me for my exertions on behalf of our Society. I only regret that they are not more valuable than they have been ; and the success

of the Institute has been an ample reward for all who have worked for it. With reference to what Professor Macdonald has said, I think, when he reads the Address, he will see that I have not written against the Anthropological Society as such, but merely have given a specimen of one of its papers. It would have occupied too much time on that one subject, or I might have gone on to show, that the first member of the Anthropological Society who spoke on Mr. Heath's paper—a man well entitled to speak on the subject,—I mean Mr. Luke Owen Pike, who is the author of a recent able work on the British People, condemned the paper very much more thoroughly and in rounder terms than could possibly be implied from my comments. The only other speaker whose speech is yet reported on the subject, did advocate its conclusions, as a Darwinian ; but I could not enter into their criticisms of the paper ; and I only gave the extracts to show the way in which the subject was treated in that Society in contrast with our own ; but not wishing to identify the Anthropological Society with Mr. Heath's opinions, for I believe when the whole discussion comes out, it will be found that there is scarcely a single person who entirely agrees with him.

The Rev. W. MITCHELL now left the Chair, which was taken by Captain Fishbourne.

Rev. W. R. COSENS.—I feel I am highly honoured by having had the next motion placed in my hands. It is one which will require few words from me to commend it ; but at the same time I think whoever moves it, ought to pay that due respect to the person whose name is contained in it which we all feel for him. I am a young member of the Society, and I come here to learn, in order that I may be able to teach, I hope more truly and thoroughly, those who are committed to my charge,—my poor people in Westminster. I am sure all of us may derive from the excellent and eloquent addresses that have been delivered from time to time by our Vice-President that instruction which, I may say for myself, I have received from them. (Hear, hear.) I beg to move a vote of thanks to our Chairman ; and I am sure you will all agree with me, that not only this evening, but on every occasion when Mr. Mitchell is present, he gives a right tone to our thoughts, by those few words he may speak, or the addresses with which he may favour us, indicating the strict line which we ought to adopt with regard to those very abstruse subjects we have to consider. I feel it is a privilege for us that such a man should preside here so constantly, so that we have the benefit, not only from time to time or of an occasional attendance of so learned and gifted a man, but that he devotes his time and attention almost without limit to this Society. I feel, therefore, we ought to record our cordial thanks, not only for his presiding this evening, but for the constant amount of labour and thought he devotes to the interests of this Institution. (Hear, hear.)

Captain F. W. H. PETRIE.—I beg to second that motion. I was among the first who joined the Victoria Institute, and having worked as well as I could at it, I can bear my testimony to the great value of the services of Mr. Mitchell. (Applause.)

The CHAIRMAN.—I need hardly put this resolution to the meeting. I am

sure you will all entirely concur in it. I have pleasure in also bearing testimony to the great value of our Chairman's services ; and I may say I know that his addresses have had considerable influence outside our Society. Our noble President has told me, that in the circle in which he moves, he hears constantly that the effect produced by those addresses is considerable indeed ; and I am sure you will give your unanimous vote of thanks to Mr. Mitchell for presiding on this occasion.

The motion was carried with acclamation.

Rev. W. MITCHELL.—It gives me the greatest possible pleasure to work for this Society. I feel it to be a labour of love, as well as a duty ; but that labour is very considerably lightened by the assistance I so constantly receive from our Honorary Secretary, and also from our worthy Treasurer. They do everything they can to lighten any little work I undertake, and I can assure you it gives me the most sincere pleasure, on every occasion when I have an opportunity of meeting you, and of furthering the interests of this Institute, which I believe to be identical with the best interests of our race. (Applause.)

This concluded the business of the Meeting.

ORDINARY MEETING, JUNE 3, 1867.

CAPT. E. GARDINER FISHBOURNE, R.N., C.B., HON. TREASURER,
IN THE CHAIR.

The minutes of the previous meeting were read and confirmed.

The Rev. WALTER MITCHELL, M.A., vice-president, then delivered a lecture on "The General Isomorphism of all Crystalline Bodies, and the Relations of all Forms of Crystals to those of the Cubical System," being an outline of a paper on this subject which will be hereafter published in the *Journal of Transactions*.

[Mr. Mitchell's paper has now been partly prepared, and it was expected that the first part of it would appear in the present number of the JOURNAL; but owing to the tables of minute figures, and the diagrams illustrating the paper, which will delay its passing through the press, I have deemed it advisable not to keep back this number of the JOURNAL any longer, but rather to defer Mr. Mitchell's important Memoir, which will be published when completed.

The subject was one that scarcely admitted of *extempore* discussion; but the following observations were made upon the occasion when Mr. Mitchell's lecture was delivered; and the remarks of Mr. Charles Brooke, F.R.S., now a vice-president of the Institute, and of Professor Morris, F.G.S., of University College, London, will be read with interest.

In the course of his lecture, Mr. Mitchell had especially referred to the valuable labours of Mr. Brooke's father in the same field, as well as to those of Haüy, and others, who had endeavoured to discover the true laws of crystallography.—J. R., Ed. February, 1868.]

The CHAIRMAN.—You will allow me to give our unanimous thanks to Mr. Mitchell for his very interesting lecture. Few of us can follow him, I am sure, throughout the whole of it, but we can all follow him so far as to see that there is a manifest design in these laws of crystallography, and that there must be a very large amount of increasing credulity among those who, in the face of such knowledge, still deny both a design and a Designer in nature. It is quite impossible that such laws, ranging themselves so indisputably and clearly under one great law, can be considered accidental, and as

the result of fortuitous circumstances. There is clearly a design and a Designer throughout all ; and I am sure we have learned a great deal more, and seen a great deal further, than we could have done without Mr. Mitchell's assistance. He has furnished us with a fresh proof that can leave no room for doubt in our minds as to the evidence of a Designer—of a great Author, ordering all things for His own purposes. There are several gentlemen here, I understand, conversant with the matter—Mr. Brooke, whose name has been so often mentioned by Mr. Mitchell, and Professor Morris—and perhaps they will give their opinions on the subject.

CHARLES BROOKE, Esq., F.R.S.—I beg leave to observe that the name mentioned was the name of my deceased father, and not of myself ; but I have also paid a great deal of attention to this subject, and am extremely interested with the views Mr. Mitchell has so ably worked out. The views contained in the paper my father published were just those which Mr. Mitchell represented them to be ; and some of those views he has now enunciated, certainly very cleverly, carry the mutual relations of the various forms of crystals to a still further point than they were developed in my father's paper.

Professor MORRIS, F.G.S.—I find it difficult to offer any additional remarks this evening, after hearing the very learned lecture of Mr. Mitchell, but I think it is one of those subjects which although apparently difficult at first, yet, to those who have paid attention to it, becomes in the end most clear ; and one of the great merits of Mr. Mitchell's researches is that his labours will tend ultimately to simplify the difficult science of Crystallography. Few persons are aware when they look round our public collections, seeing the varied forms of crystals, which are the flowers of the mineral kingdom, that they all belong to some great law, marking design ; and I think that the explanations Mr. Mitchell has been giving, point out that there is as much evidence of design in the mineral kingdom as we find in the law of symmetry in the vegetable kingdom. (Hear, hear.) There are certain types in the vegetable world by which are arranged the great classes of plants ; and now we have seen that there is also in the mineral kingdom another and analogous law. I think it must be a great gratification to this Institute to have been the means of inducing Mr. Mitchell to explain those broad principles which were seen by others to some extent, but which he has completed and marked with a master's hand. I think it will be also interesting to those who are not acquainted with crystallography, to know that every mineral substance, as Mr. Mitchell pointed out, follows some definite type in crystallizing. I may observe, also, that that beautiful law which he has explained with regard to minerals has its practical value : it is valuable in the cabinet, and also commercially, as it enables us to detect the true from the false, as well as how to clear off the blots from our beautiful gems. Many of you are, perhaps, aware of the law as it affects the diamond—and which Mr. Mitchell is especially familiar with—its peculiar cleavage ; and how some diamonds, on obtaining a better form by being scientifically cut, have been afterwards valued at a much higher price. We have one case

where the purchaser of a diamond worth some thousands of pounds, took it back to the seller after cleaving it according to the laws of crystallography, and I think it realized a thousand pounds more. We are still more indebted to this knowledge, and especially to our lecturer this evening, for I believe it was owing to his suggestions that the Koh-i-noor, which was but roughly cut when it was brought to this country, afterwards had its flaws removed and its beauty increased. (Hear, hear.) I think all of us must feel that this apparently difficult subject has been made very interesting to all, since it has been shown that those beautiful flowers of the mineral kingdom have laws equal in regularity to those of the other kingdoms of nature, and that crystals are all connected with one fundamental form, taking the cube as the primary form, and that the other forms are derived from modifications of this figure. What Mr. Mitchell has discovered and pointed out is like to the sculptor, who seems to realize, even in the solid block of marble, some beautiful image hereafter to be chiselled out ; for Mr. Mitchell has shown us how, from the common cube, all the beautiful and varied crystallised forms found in the mineral kingdom may probably be derived. (Hear, hear.)

MR. REDDIE.—It is a fortunate circumstance that our chairman this evening is also our treasurer, because I think I shall only express the sentiments of the Council, as well as of all our members, in saying that we must endeavour to do every justice to the valuable and important paper of which Mr. Mitchell has now given us an outline. And I propose that in this instance we shall depart from what has hitherto been our practice, and produce Mr. Mitchell's paper when written, not only with illustrations, but with coloured illustrations, giving as far as possible the effect of the coloured models with which he has illustrated his lecture, and by which the relationship of the different classes of crystals to one another has been made so very distinct, and, I had almost said, so very simple. I am sure, if you agree, that the Council will be most glad to allow, though at some expense, a page or two of coloured illustrations, in order to do justice to this most valuable paper on crystallography. (Approbation.)

The meeting was then adjourned.

ORDINARY MEETING, JUNE 17, 1867.

THE RIGHT HON. THE EARL OF SHAFTESBURY, K.G., PRESIDENT,
IN THE CHAIR.

The Minutes of the previous Meeting were read and confirmed.

The HONORARY SECRETARY then announced that the following members and associates had been elected, viz. :—

FOUNDATION LIFE MEMBER :—Joseph Ince, Esq., Assoc. K.C.L., F.L.S., &c. &c., 26, St. George's Place, Hyde Park Corner.

MEMBERS for 1867 :—The Rev. Charles Graham, Shepherd's Bush ; The Rev. John Jennings, M.A., Canon of Westminster, Rector of St. John's, Westminster ; Samuel Morley, Esq., Craven Lodge, Stamford Hill ; E. B. Savile, Esq., 305, St. George's Road, S.W. ; John Vanner, Esq., Eliot Vale, Blackheath ; T. H. M. Walrond, Esq., H.M.C.S., 37, St. George's Road, Eccleston Square.

ASSOCIATES, 2ND CLASS.—Hunter Alex. Coghlan, Esq., M.R.C.S. Lond., Army Medical Staff, Castle Hill Fort, Dover ; the Rev. J. R. Pursell, M.A. Oxon, Angersholme, Fleetwood ; Rev. Henry Sharpe, 31, Belsize Road, St. John's Wood.

It was also announced that the following books had been presented to the Institute, viz. :—

"The Inspiration of Scripture : Its Limits and Effects." By George Warington, Esq., author of "The Historic Character of the Pentateuch Vindicated," &c. &c. *From the Author.*

"Actonian Prize Essay on the Phenomena of Radiation." By the same. *From the Author.*

MR. REDDIE.—I regret to say, my Lord, that I have a communication to make which will no doubt cause some disappointment to all present. Professor Kirk had fully intended to be here this evening to read his paper. He had come up from Scotland, and had been several days in England with this view. But while on a visit in Kent he was informed of the death of a very dear friend, Sir Wilfrid Lawson, and was also requested to attend the funeral, which took place to-day in a northern

county. Under these unavoidable circumstances I have arranged with the learned Professor to do my best to read his paper for him, so far as time will admit of its being read, and to explain the main drift of the passages which, on account of its length, it will be necessary to leave unread. I shall endeavour to do justice to Professor Kirk's thoughtful essay, which I have carefully read through; and I have only further to express his extreme regret that he could not be with us this evening, and I beg leave to add my own regret at the cause of that absence.

The following is Professor Kirk's paper, which was then partially read by the Honorary Secretary :—

ON THE RELATION OF METAPHYSICAL AND PHYSICAL SCIENCE TO THE CHRISTIAN DOCTRINE OF PRAYER. *By the Rev. JOHN KIRK, Professor of Practical Theology in the Evangelical Union Academy, Glasgow; Author of "The Age of Man Geologically considered in its bearing on the Truths of the Bible," &c. &c.; Mem. Vict. Inst.*

WHEN we speak of Physics and of Metaphysics to ordinary men, we have reason to fear that they are sensible only of certain unintelligible sounds. If their thinking capabilities are at all brought into use, it is merely in the perception of a *mist* which has risen before the mind's eye. Should we expatiate technically on Psychology, or Biology, or Anthropology, launching off perhaps into ever so many other "ologies," the fog only becomes more dense and murky, till the baffled hearer becomes hopeless as to all understanding of that which is addressed to him. The simple-hearted no doubt imagine that we who use these very learned words must understand ourselves, and see all beyond the clouds and darkness which limit their view; but they often *admire* when, if they only knew the real state of the case, their estimate would be very seriously modified. It is in this state of ignorance and simplicity that the common mind is especially in danger from popular philosophy.

There is nothing in the nature of the highest knowledge which renders such ignorance necessary, even in the most lowly of ordinary men. The facts and findings, which go to make up the Science of mind itself, are not so mysterious or incomprehensible in themselves that the intellect of the many may not embrace them. Neither are the facts and inferences, which constitute the knowledge of matter and its laws, so much beyond the common range of thought that they may

not be reached by any one who can understand good plain English. It is required, we think, only that both these kinds of knowledge shall be expressed in such language. And we are strongly disposed to believe that such an embodiment of truth, in easily intelligible words, is as necessary to the real knowledge of the Philosopher as it is to those whom he would teach. As the modern mathematician makes his symbols "think for him," so we fear do some of the most noted men in other departments of Science, allow mere phrases to do the work which belongs to clear and careful thinking. By this they deceive themselves as much as they mislead others, and perhaps even more. Mr. Stuart Mill well says that "the mere forms of logic and metaphysics can blind mankind to the total absence of their substance."* This is strong language, written too by a philosopher of philosophers, and not of common men; but it is sadly true.

In endeavouring, therefore, to make our consideration of this great subject really useful, I will do my best to make my meaning clear and accessible to the common mind. Not that I think this possible without some degree of earnest industry on the part of those who read that which is written, but that all who are willing to give a moderate measure of effort on their part shall enjoy the fruit of that effort in a somewhat increased possession of the truth.

By Physical Science I understand that thought by which material objects are truly represented in the mind. Not, however, such thought as merely agrees with these objects as they exist in nature, but such as is *known* thus to agree. What are called "*hypotheses*" are thoughts which in some cases agree with the objects to which they are related, but so long as they are "*hypothetical*" they do not belong to science, properly so called, inasmuch as they are not knowledge. Reason has as yet failed to lay hold on them—they live only as conjectural notions in the imagination. I cannot help thinking that all such thoughts should be considered as alien to really scientific investigation.

By Metaphysical Science I understand that true thought which represents all such objects as lie above and beyond the material. The student of *pure Physics* has strictly speaking no thought of mind. The student of *pure Metaphysics* has no thought of matter—all his reasonings are of *thought itself*. The student of *truth* takes equally earnest care to deal with all thought which stands to reason, whatever the object of

* *Examination of Sir William Hamilton's Philosophy*, p. 61. Ed. 1866.

that thought may be. His grand aim is to make sure of all thought which corresponds with that which is real, and he finds that he cannot do this without learning of matter as well as of mind, and of mind as well as of matter. To trace, then, the relations to which, in this paper, our attention is directed, we must look candidly and with deep earnestness into all thought of realities which bears upon the doctrine in question.

By the Christian doctrine of prayer I mean neither more nor less in this paper than *man's asking—God's giving as the consequence of that asking—and man's receiving as the consequence of that giving*. The point of thought specially in view is that of GOD'S GIVING, in consequence of man's asking. Our inquiry will virtually be as to whether Metaphysical and Physical Science, in their grand results, are consistent with this idea of God's acting in direct and real consequence of man's asking. No one who knows the influence of Science on the one hand, and of real prayer on the other, will fail to see the vast importance of such a subject. It is philosophical, yet eminently practical, and even, as "divines" would say, "experimental." I mean to treat it as almost, if not altogether, a subject in philosophy; yet as one of those many subjects in philosophy which necessarily thrust themselves into the domain of religion. My aim is to show, how perfectly true Science ever bears out true theology and also true life in man.

In an inquiry like that on which we thus enter, it seems necessary to make as sure as may be that we understand the true nature of knowledge itself. Science is knowledge, but we need to ask what it is "*to know*." This is in itself a vital point in metaphysical investigation, and so forms an appropriate introduction to all that follows. The philosophical writer whom I have quoted above gives us incidentally one of his ideas on this point. Speaking of the inmost nature or essence of a thing which he argues "we cannot know," he says—"If there were such a central property, it would not answer to the idea of an 'inmost nature,' for, if knowable by any intelligence, it must, like other properties, be relative to the intelligence that knows it—that is, it must consist in impressing that intelligence in some specific way, for this is the only idea we have of knowing; the only sense in which the verb 'to know' means anything."*

I must remark, with great humility, that this is far from tolerable English. "A property," we are told, "must consist"

* *Examination of Sir William Hamilton's Philosophy*, p. 14. Ed. 1866.

not in an impression, nor in a capability of impressing, but in "impressing" itself! A noun must be a verb! A quality must be an act, if not a process! And this strange property, or stranger act, is our only idea of knowing! This unmasterable confusion of words must express the only sense by which the familiar verb "to know" has any meaning! We can only guess that Mr. Mill meant to say, that an impression on the intelligence, made in some specific way, is the only meaning of knowledge. But is it so? Would not this notion of knowledge, by taking personal activity out of the idea of knowing, invert the verb "to know?" If that verb does not mean something which is mentally done by the person who is called an "intelligence," but only an impression which is made upon that person, is it not then absurd to say, as every man does when he has satisfied his reason on any point, "*I know?*" Warmth from an external object is simply an impression which that heated object makes upon me; speaking of that impression would it do to say "I warm" when the whole truth is that I am warmed? Or, to take a stronger instance, if pain is produced in me, that is an impression made on me as a sentient being—would it do to say in such a case "I pain," when the truth is I am pained? So, if knowledge is only an impression made on me as an "intelligence," or as an intelligent being, can it be right for me to say "I know," when the truth is I am impressed? If I am only impressed, I am passive; and it must be absurd as we shall yet more fully see, to say I am active, as I doubtless do say, when I use the words "*I know.*"

This is a very important point of truth, and worthy of our best attention, when careful to see the relations both of Metaphysical and of Physical Science. We must, therefore, make as sure of it as we can. An intelligence, as already indicated, is a person. There is no such thing in being, by itself, as an intelligence which is not a person. Intelligence by itself has no existence. It is only the capability of knowledge, belonging to a *being* who is thus capable. An impression on such an intelligence, therefore, is an impression on such a being or person. We are acquainted with at least three kinds of impressions that are made on such a person, in regard to which we should think all are agreed. According to Locke's system of philosophy, which Mr. Mill follows closely, only two of these kinds of impressions are possible as coming directly from the external world, and neither of them is knowledge, nor are both combined that which is properly called by that name. There is one kind of impressions that are made upon the body without their affecting the senses. The patient, for

example, undergoing an operation under the influence of chloroform, is impressed, and writhes, groans, and even screams, as if sensible of agony, and yet is all the while totally unconscious. This impression made on the person is (certainly enough) not knowledge. There is another kind of impressions, which are made when the senses only are affected, but neither is this properly knowledge. Pain, however acute, and pleasure as mere sensation, however pleasant, is not knowledge. Knowledge is thought, but pain and pleasure, merely sensational, are not thought. Such a state as mere sensational consciousness is no doubt an impression upon an intelligence when the sentient being is intelligent, but it is not an impression on the intellect as such, but on the mere sense, and hence it never is properly called knowledge. This truth is fatal to Mr. Stuart Mill's idea as we have quoted it. If there is no "sense" for "the verb to know" but that of an impression made by a material object on an intelligence, then, if we adhere to what we shall yet see to be Mr. Mill's own notions, there is no sense, in truth, for the verb at all!

Mr. Mill is, as we have said, a follower of Locke in the fundamental ideas of what may be called his system of thought; though the additional light which has fallen on philosophy since Locke's time *will* mix itself with the darkness that broods over those who are yet in bondage to his views. Locke's great principle was that "*all ideas come from sensation and reflection.*" He says—"Our observation, employed either about *external sensible objects, or about the internal operations of our minds, perceived and reflected on by ourselves, is that which supplies our understandings with all the materials of thinking.*"* There is much more indicated here than "impressing the intelligence in some specific way." Reflection, or the throwing back of the mind on impressions that have been made upon it, is found in activity, not in passivity like impressions. Yet there is a very serious gap in Locke's system. He says, "Material things as the objects of sensation, and the operations of our own minds as the objects of reflection, are to me the only originals from whence all our ideas take their beginnings." These words make Locke's great mistake very evident. If we carefully observe the facts of the case we shall find that to speak of an "*object*" of "*sensation*" is to speak inconsistently with truth. Sensation, strictly speaking, has no "*object*." It has a cause in the external object by means of which it is produced, but that is not an object to the sensation nor to the man as merely sentient—it

* *Essay on the Human Understanding*, Vol. I., pp. 67, 68. Ed. 1753.

is an object to the man only as intelligent. If, for example, I am dazzled with light, I have a powerfully produced sensation caused by means of the radiance of some luminous substance; but that substance is not in such a case an object to me at the moment. All other capabilities of mind are absorbed in the one state of sensation. I cannot see from the excess of light,—that is, I have no power of attention even, from the overpowering degree of the sensation. In the same manner excessive pain destroys all intelligence in the agonized individual during its continuance. Sensation absorbs him so that he can think of nothing; at certain moments, not even of the pain.

It is, consequently, a fundamental error to speak of the "object" of a sensation. It gives rise to Locke's great error in which he confounds sensation and perception. He regarded reflection as confined in its objects to the states of a man's own mind, and by reflection he means all else in the soul's knowing states besides sensation. He overlooked the fact that sensation in itself fails to connect the intelligence with external objects, and so he reaches the notion that all our knowledge of the external world is "*mediate*"—that it is, in fact, the knowledge of our own states of mind merely. He calls these states of mind with which reflection has to deal "*ideas*." By real ideas he means "such as have a foundation in Nature; such as have a conformity with the real being and existence of things, or with their archetypes."* Locke had a wholesome hatred of "innate ideas," and he may be said to have abolished them; but he remained the dupe of the notion which regards such ideas as we acquire, in the light of *things* or beings, that may become OBJECTS of contemplation by themselves. Our knowledge of the external world is, according to this system, only our knowledge of these ideas. Locke found himself landed in real difficulty by means of this notion when he came to the point of "*reality*" in our knowledge. He owns the difficulty. He proposes the question—"How shall the mind, when it perceives nothing but its own ideas, know that they agree with things themselves?"† *That is, how shall we compare two things when we have no knowledge beyond that of one?* He most truly says—"This thought seems not to want difficulty." He endeavours to remove the difficulty in a way in which he must have felt the weakness of his own reasoning. It is not necessary that we should follow him in his effort. It is not true, in fact, that our thought of external objects is *mediate*. The states of our own minds are not the only, nor

* Essay, Vol. I., page 6.

† Essay, Vol. II., page 186.

are they the chief objects of those states of mind which Locke includes in reflection. To see this we have only to notice that sensation does not, as a general rule, call attention to itself, but to an external object. A man who, like Locke, has got his mind twisted into a certain direction of thinking, fixes his attention on the sensation rather than on the external object which produces it, but this is an exception to the rule of human thought. That thought, so far as the material world is concerned, is *not* of sensations, but of sensible objects. It is not mediate but immediate—it is at least as immediate as sensation is immediate.

If one follows this mediate school of mental philosophy, he is led to think of the mind as a pool which is full of fishes, one class of which preys upon another. All that ranges under reflection lives upon all that ranges under sensation. It is lost sight of that in all thinking one state of the mind is exchanged for another. That which is now only sensation, is the next moment attention to the object that has given the sensation. You may say, perhaps rightly, that it consists of two elements, and is of the nature of both sensation and attention, but that does not make it two states of the mind. It is, in fact, only perception, and perfectly distinct from mere sensation. It must also be observed that no one can establish the mediate character of our knowledge by saying that sensation is always first and reflection after. You may as well say, because I see a thing first, and then feel it, my feeling is mediate, while my seeing is immediate. The feeling in such a case is second in order to the seeing, but both are equally immediate. Certainly the one is not through the medium of the other. Just so with attention and that thought of an external object, which sometimes goes before the sensations which that object is fitted to give,—as it often follows some of these sensations. It is true that sensation depends on organs of sense which are part of the external world, but that can never establish the doctrine that thought of this world is thought of our sensations, for all our thoughts depend on organs of thought that belong to this world too. In the history of our states of mind, so far as the material world is concerned, sensations are first—thoughts follow—but neither does that determine that sensations are the only objects of thought, any more than that a person who should hear before he could see would thereby see nothing but his hearing. In cases in which an object gives me sensations first, these sensations, as a rule, are followed by attention to the object (not to the sensations), but the state of mind which amounts to thought of that object is as directly connected

with the object itself as it is possible for the sensations to be. If we are desirous to *know—to make sure* of an external object as we express it—we examine it by means of our senses. We do not examine our sensations, nor do we examine exclusively by means of our sensations. We examine the object by means of the sensations, and also by means of all the other states in which the mind can be brought to bear upon it. You might as well say that a mechanic is working, not upon the machine which he is constructing, but upon some of the tools with all of which he is constructing it, as say that we are reflecting upon our sensations, or our ideas, or both, when thus endeavouring to reach a real knowledge of this object.

I am not, however, to be understood as meaning to argue that our direct thoughts of external objects are knowledge any more than are our sensations. My aim here is to show that we must seek for that which may be truly called knowledge in something else than the mere impressions which are made upon us by the objects of that knowledge. It is to be remembered also, that impressions are as real when made directly on the mind itself as when produced through the organs of the body. The thought which takes place in the man when no external material object whatever is producing any impression on the body, or on the senses, is an impression as real as any sensation that is ever experienced. The facts of mere consciousness, observes Cousin, "can be observed quite as well as those which take place on the scene of the world. The only difference is that on the one hand they are exterior, on the other they are interior, and that, the natural action of our faculties carrying us outward, it is easier for us to observe the former than the latter."* Yet every fact of direct thought in consciousness is not, properly speaking, knowledge. If, for example, a gold-digger in one of his reveries has the thought of a large nugget, which lies hid in a certain piece of rock, raised in his intelligence, or thinking self, and as the result of that thought he goes and finds a nugget in a rock which he never saw or heard of before, it would be very difficult to prove that this thought was produced as either a direct or an indirect impression by the rock in question; but the thought is a real impression on the intelligence. It is in harmony, too, with the object thought of, yet no one will call that impression on this intelligence by the name of knowledge; nor can any one take all the facts of our mental history into account and leave out such directly suggested thoughts. You cannot say that "*the digger*

* Cousin's *History of Philosophy*. Second Series. Vol. II. Lect. XVI.

knew the nugget was there," and yet the true thought of it was in his mind exactly as if he had seen it with his eyes.* We are thus forced away from this notion of knowledge as an impression on an intelligence. No mere impression made on the intelligence in any way in which such an impression ever is made is really knowledge, or the true meaning of the verb "to know;" since even true thought directly impressed on the mind is not knowledge. All these impressions, outer and inner, are but the *raw material*, so to speak, from which knowledge is manufactured.

Mr. Stuart Mill himself gives us the key to another idea of knowledge when he says that—"What consciousness directly reveals, together with what can be legitimately inferred from its revelations, composes, by universal admission, all that we know of the mind, or, indeed, of anything else."† Here manifestly are two very different classes of ideas—direct revelations of consciousness, and *inferences legitimately derived* from these revelations. Whatever is to be understood by such revelations, it must be distinct from the inferences. The first may be impressions made upon the mind; but the second are results produced by the mind's own working and are not mere impressions. There can be no confounding of these two classes of the states of every man's mind, by any one who is careful to think clearly on the subject of knowledge. But there is more than their differing from each other to be noticed, of these classes of mental states. Sensations by themselves, coupled with direct ideas that rise in the intelligence, form a momentous assemblage of such states; but neither the one, nor the other, nor both, as we have already seen, can reasonably be set down as knowledge. It is only when that has taken place which is expressed by the words "*I infer*," that

* We might take such cases as the following to illustrate this point. A friend of mine was engaged in a lawsuit which cost him great trouble. About a year before it was settled he saw in a dream the postman coming to him with a letter telling him of his success, and he imagined that he brought it in and read it to his wife. The dream was a *perfect representation* of what took place when his agent wrote to him of the termination of the suit. No one would say he *knew* a year before what would occur, and yet he *saw* it all. Another friend has handed me a letter in which a husband says, that on account of his wife's extreme weakness her brother's death had been concealed from her, yet, he says, "she has seen him on his deathbed, and also seen him die." It was all to her as if she had been there, yet they told her it was "only imagination," and she could not say whether they were telling her the truth or concealing it. She could not be said to *know* he was dead.—J. K.

† *Ex. Sir W. Hamilton's Phil.*, pp. 107, 108. Edit. 1866.

we are within the region of that which is rightly so named. As a far abler writer than Mr. Mill says—"Without reflection, man would play only a feeble part in the perception of truth; he, indeed, takes possession of it, he appropriates it to himself only by reflection."* And, as a yet more celebrated writer than either has said—"Whether we wake or whether we sleep we should not suffer ourselves to be persuaded except upon the evidence of our reason. Observe, I say of our *reason*, not of our imagination, or of our senses."† Even if we think of "that which consciousness directly reveals," we must think of something else than sensation, or we cannot find anything in it which can be properly called knowledge, unless we are prepared to confound sensation and thought, and so to make knowledge a matter of the passive senses, instead of a matter, as it is, of the active intellect. And if we are to think of direct suggestion as knowledge, we must, I fear, confound mere vivid thinking with true knowing. Multitudes of the thoughts which at one time are so clear and strong in us that we imagine we know their objects if we know anything at all, turn out to be only delusions. How shall we distinguish between these and those direct thoughts to which we may rightly give the name of knowledge? How shall we even conclude, or know, whether a direct thought is a true intuition or only a fleeting fancy? If we should take the mere thought of personal existence expressed in the "*I*" or the "*me*," how shall we know that this is not a mistake? *We must compare and infer.* Apart from this comparison and inference there is, no doubt, thought: but all thought—even all true thought—is not knowledge. Consciousness supplies us with occurrences—matters of fact as occurrences in us—impressions, if you so choose to call them—myriads of impressions in relation to both the outer and the inner worlds, but these, as they are directly supplied, are not knowledge. They must be compared, sifted, and wrought out into thoughts which are the product of reason, or they can never bear the sacred name in a proper use of terms.

It is in this process of comparison, sifting, and working out, that we light upon a full conviction of the truth, that there are two great classes of substances in the universe—the one we call *matter* and the other we call *mind*. But here we encounter a most formidable objection already alluded to incidentally. It is denied that we know any such thing as *substance*. Here we

* Cousin's *History of Philosophy*. Second Series, Vol. I., Lect. VI.

† Descartes' *Discourse on Method*.

find the great importance of having settled the meaning of the verb "to know." Berkeley is the great teacher of the non-existence of material substances as such. He says, "It is indeed an opinion strangely prevailing amongst men, that houses, mountains, rivers, and, in a word, all sensible objects, have an existence natural and real, distinct from their being perceived by the understanding. But with how great an assurance and acquiescence soever this principle may be entertained in the world; yet whosoever shall find in his heart to call it in question, may, if I mistake not, perceive it to involve a manifest contradiction. For what are the forementioned objects but the things we perceive by sense, and what do we perceive besides our own ideas or sensations; and is it not plainly repugnant that any of these, or any combination of them, should exist unperceived?"* It is not difficult to see where the foundation of this absurdity lies. Berkeley, in following Locke, takes it for granted that we have no perception of external objects, but only of sensations and ideas in our minds. He has no thought that perception may be a state of the mind as truly and directly produced and maintained by an external object as sensation. He does not even imagine that a sensation itself can be only a temporary state of the sentient being, produced by means of an external material object. If he admits even this, his theory is gone, for the external object must exist in order to its being the means of producing the sensation. It is not difficult, we think, to explain satisfactorily this "strange impossibility," which the philosopher says stood in the way of his even imagining the existence of the world apart from his sensations and perceptions. The pool full of fishes is a fair illustration of his case. He had committed the mistake of imagining sensations and ideas as realities in themselves, and not merely modes of himself as a sentient and intelligent being. He had admitted an ideal world consisting of these sensations and perceptions to come between him and the real world with its "everlasting hills." He had allowed this ideal world to become so vivid and fixed in his imagination that he could see nothing through it. His illusion was so perfect that there was not any thing in his philosophic universe of a real nature but this ideal dream itself. Yet this mistake ought not to mislead any careful thinker. We have in man a being capable of affections from matter, which we call sensations—but capable also of affections from the same matter, which we call ideas, thoughts, perceptions—these affections being nothing more or less than states of

* *Berkeley's Works*, Vol. I., p. 25, edition 1784.

that man himself, resulting from his meeting with material objects and dealing mentally with them. We may as well say that he sees only his seeing, as that he knows nothing beyond the states of his own mind.

Berkeley, as we have seen from his own words, was an unhesitating assertor of the non-existence of material substance, as such. Some modern authors, who follow in his track, are more cautious—we should say timid—in their declarations of his doctrine. Following the principle of “know-nothingism,” which is so acceptable to many, they only say that we do not know such a reality as matter. John Stuart Mill states, as the popular doctrine, that “*all we know of objects is the sensations which they give us, and the order of the occurrence of these sensations.*”* In another sentence he says that, “It may, therefore, safely be laid down as a truth, both obvious in itself and admitted by all whom it is at present necessary to take into consideration, that of the outward world we know and can know absolutely nothing, except the sensations which we experience from it.”† It will be observed that there is a manifest inconsistency between these two sentences—the first admits the “order” of our sensations as well as the sensations themselves; the second excludes “absolutely” that “order.” It will be seen also that they are both utterly irreconcilable with that description of knowledge which we have before quoted from Mr. Mill, when he says that it consists of the revelations of consciousness, and *all legitimate inferences* derived from these revelations. We have, surely enough, consciousness of more than sensations, and legitimate inferences of more than their order of occurrence in the mind. But there is also marked inconsistency between this language as to knowledge, and the first which we quoted from Mr. Mill—that the only sense in which the verb “to know” has any meaning is found in the properties of objects impressing the intelligence. Sensations, as we have seen, are not impressions on the intelligence, not even on an intelligent being as such. They are impressions on the senses only—that is, impressions on a being who might have these impressions if he had no intelligence or capability of thought whatever in his nature. Where there is such confusion of ideas and recklessness of expression it is not cause for wonder that a writer should reach any sort of conclusion either in regard to matter or mind. A “sensation” is certainly not an “inference”—and the “order” in which sensations occur in the mind is not itself a sensation. Neither is any impression on the intel-

* *System of Logic*, Vol. I., p. 64, edition 1865.

† *Ib.*, page 66.

ligence as such capable of being confounded with an impression on the senses. And yet all would need to be one and the same—sensations—ideas of order—impressions on the intelligence traceable to no object capable of affecting the senses—all must be identical—in order to the consistency of Mr. Mill's statements as to our knowledge of substances.

If we rid ourselves of the confusion thus doubly and trebly confounded in the tangled thinking of so-called philosophy, we meet a question which accords with at least one of Mr. Mill's ideas of knowledge.—Is our inference, that substance exists and that qualities are only modes of the being of this substance, a legitimate inference? If it is so, then we *know* that substance does exist, and that sensible qualities are only modes of its existence.

But here it is necessary to be careful that we really understand what we mean by a *mode* of existence. We get at this by passing from the mere abstract idea of a *mode*, or *manner*, to the concrete idea of the mode or manner of being in a particular object. Let us take the case of an elastic ball and its form. It is round, in the form of a sphere. Press it between the finger and thumb, and it is no longer of the same form. It exists at first in the mode of a sphere—then in that of another figure—and when we let it resume its first mode it exists in that again. These mere changeful modes of being are *nothing* apart from that whose mere modes or manners they are. The inference, as to whose legitimacy we are inquiring, is, that the form of the ball is not itself the ball. Here again we come upon another phase of the question.

May I legitimately infer that the affection of sense in me is a mode of my being caused by an external object? If I take up a piece of gold and bring it before my eye I have the sense of its yellow colour. If I remove the gold from the range of vision I have the sense of yellow no longer. If I repeat this double experiment millions of times I have each time the same result. Is it a legitimate inference that this piece of gold is capable of giving me this sensation of a yellow colour? If such a thing as a legitimate inference can be the result of the most perfect induction this is such an inference. What I call yellow in a bit of gold, is only a certain form or arrangement of particles in that gold in virtue of which it transmits the motion which I call light in a particular way—it is only a mode of being in the gold, and the sensation of yellow is only a mode of being in me. The mode in the gold is answered by the mode in me, and as the gold invariably gives me the sensation, I infer that it is invariably capable of doing so. But a capa-

bility is not a thing—it is, as we have seen, only a mode of existence in a thing—the yellow colour is only a mode of existence in the material gold. The whole controversy turns on the *legitimacy of the inference* that there is something which has this mode of existence.

For further illustration let me take up a piece of red-hot iron. Heat is only a mode of existence—as we say a *state* of this iron—it is in fact a mode of the nature of motion. Is it not a legitimate inference from the facts of consciousness, caused by the presence of this hot iron, that there is present in it a *substance*, which is in a state of motion that we call heat? If the inference which replies in the affirmative is legitimate, then we know that there is a substance which we call iron. Connected with this piece of iron there may be any number of facts in sensation, and all these facts in a given order. Do these facts in that order direct the intelligent thinker to infer the existence of the substance? If they do, and that according to all the laws of truth, so that from this direction he concludes that substance is not quality, but that of whose existence all qualities are only modes, then he *knows* that substance exists, and that it exists in these states or modes which we call properties or attributes. To meet this, he who asserts that substance does not exist, or that we do not know that it exists, must show,—and that by an induction of facts more influential than that induction which is opposed to him,—that his inference is legitimate. He must thus overturn the cogent reasoning by induction on which the common sense of mankind itself is based, if he would legitimately keep to his strange ground. We conclude, therefore, that there are substances in the universe, and go on to say that there are two great classes of substances—the one called *matter* and the other *mind*. We are thus led to inquire as to the modes of existence belonging to those substances, or, as some choose to say, their “affections,” so as to understand their relations.

Taking our knowledge as consisting in legitimate inference, and one of the most legitimate of all inferences being that a substance which we call matter exists, and also another called mind, we are met with the question as to how we legitimately infer this distinction of substances? We reply, generally, by a careful comparison of the facts as these become accessible to us. We discover by legitimate inference that the modes of existence belonging to matter are totally different from those which belong to mind. That object whose characteristic mode of existence is found in *feeling* (understanding the word in the so-called philosophic sense) is legitimately distinguished from all objects whose characteristic mode of existence is insensibility.

If I consider the facts accessible to me in relation to a marble statue of a child, and compare them with those accessible facts that relate to a living child, I find it impossible to conclude that the perfect unconsciousness of the statue and the consciousness of the child are modes of existence indicating that both are the same as to substance. Both are substances, but they cannot be similar substances. That which exists as a feeling substance must be essentially different from that which never feels but exists as an insensible thing. I examine the statue millions of times, and may have the record of millions of millions of observations recorded by others, but no fact occurs indicating that one of its modes of being is consciousness. I examine the child as often and have the record of as many observations if you will, and every one of them indicates this consciousness. I infer that there is in the child a substance whose mode of existence, being thus essentially different from that which is in the marble, cannot be philosophically or rationally confounded with the material, and I call this substance *Mind* while I call the other *Matter*. If the examination of facts may issue in the legitimate inference that an unconscious stone is not an intelligent man, because their characteristic modes of being are essentially different, so may the examination of facts legitimately issue in the inference that the substances we call matter are essentially distinct from those substances which we call mind. If a man may legitimately infer that his hat is not his shoes, because it is adapted to his head and not to his feet, then much more may he surely legitimately infer that his thinking mind is not his material body—that substances so essentially distinct cannot be identical.

Priestley may be regarded as the most prominent representative of materialism. He was preceded by Hartley, who resolved all the mysteries of thought on the principle of vibrations in the material nerves.* The materialism of Priestley is very decided. He says—"The principle of perception and thought is not a substance distinct from the body, but the result of corporeal organisation." He also says—"That mechanism is the undoubted consequence of materialism;" and again that—"The self-determining power is altogether imaginary and impossible." He has no wish to be understood within the limits of that which his language expresses. He

* See Hartley on *Man*, vol. i., page 12, edition 1749. His words are—"External objects being corporeal can act upon the nerves and brain, which are also corporeal by nothing but impressing motion upon them."

follows out the notions of his leader to their utmost limit. But we are strongly disposed to think that Priestley was more powerfully influenced by Locke than he was by Hartley—perhaps more influenced, however, by Boscovich than by either. He imagined that this naturalist had demonstrated the *mutual penetrability* of material substances. Light was then regarded as a substance, and not, as now, only a kind of motion in the molecules of substances. It is not difficult to see how this error might lead to the fancy that two particles of matter might be in the same place at the same time. Priestley says, "If the momentum of such a body in motion be sufficiently great, Mr. Boscovich demonstrates that the particles of any body through which it passes will not even be moved out of their place by it." By "*such a body*" he means one similar to light.* Now, it is quite true that "such a body" as is not "*body*" at all, but merely an agitation of the molecules of that which is illuminated, may pass through anything and not displace its particles by taking that place itself. But this is wonderfully different from a real body passing through another real body without displacing its particles, by occupying in its passage their places instead of them. It is on this *penetrability* of matter that Priestley founds his idea that spirit is material. He says:—"I therefore define it (matter) to be a substance possessed of the property of *extension* and of *powers of attraction and repulsion*. And since it has never yet been asserted that the powers of *sensation and thought* are incompatible with these (solidity and impenetrability only having been thought repugnant to them), I therefore maintain that we have no reason to suppose that there are in man two substances so distinct from each other as have been represented." † The fact that the "affections" of matter do not necessarily displace its particles, looked at under the mistaken notion that these "affections" were themselves material substances that could pass through solid bodies, without occupying their space in any degree, is the (now exploded) foundation of Priestley's whole system of materialism. The plan according to which men *refuse to know* whatever does not suit their general notions had not come into fashion in Priestley's days. It is the grand characteristic feature of the so-called philosophy of our own times. A very remarkable instance of it occurs on this very subject of materialism in Dr. Davey's book on the "Ganglionic Nervous System in the Human Body." ‡ He traces what he

* See Priestley's *Disquisitions*, page 24, edition 1782.

† Introduction to *Disquisitions*, page ii.

‡ Dr. Davey on the Ganglionic System, pages 69, 80, &c.

calls the "*formative power*" of even the highest parts of the brain to the ganglions in the sympathetic system; and when he reaches one of these, he says that beyond this some think of something which they call spirit; but he argues that there is no need for such a thing. So, in his view, all the manifestations of mind are effects of ganglionic change!

It will be observed that this is not a matter of mere distinction between qualities and substances, nor of distinction between the material and the spiritual. It is a simple denial of the existence of the spiritual being. Dr. Davey insists that the ganglion has itself the causative force by which all mental as well as material changes are effected! The metaphysician denies that we know anything of the external world but our sensations—that is, when he is in his most contracted mood, for we see he does not always shut us up so tightly; but here the physician denies that we know anything of the inner world beyond our ganglions! He holds that all that we understand by sensation, emotion, and thought, springs from these ganglions! No doubt he is quite prepared for all manner of astonishment which this monstrosity may excite, and not in the least staggered at its absurdity; so we must analyze the case as it stands in its facts. Suppose, then, that I have a handful of good gunpowder and a handful of a substance every way the same with the exception that the sulphur is absent. I put a little bit of red hot wire to the gunpowder, and it explodes; I put the same red hot wire to the other substance, but it refuses to explode. Is it not a legitimate and scientific conclusion that there is a substance in the one mixture which is absent from the other? No one in his right mind will deny the legitimacy of the inference. I may multiply the experiment millions of times, and the same result will necessitate the same inference. The experiment may be varied all over the wide field of material existence, and in every case certain results will be found dependent on the presence of certain substances. These results are modes of being belonging to those substances in certain circumstances—modes of being that can be demonstrated by experiment at all times when such experiments are possible, and that again is more than often enough for all reasonable evidence.

Take then a system of nerves belonging to a human body from which what we call "mind" is absent, and compare it by experiment with one in which what we call "mind" is present. Dr. Davey may give this "mind" any other name he chooses; just as anyone may call the sulphur in the gunpowder anything else he may fancy. Call "mind" "*formative*"

power," or call it "*life*" which is at least as good a term, the mere change of words makes no difference in the *thing*. It is not, like motion, a mere mode of existence in the nerves, it is that which gives them motion. We do not contemplate the dead brain as motionless merely, but as lacking that which once moved it. Here then is a nervous system every way perfect so far as the material is concerned, and another perhaps not nearly so perfect, yet the latter is full of sensation, emotion, and thought—are we not scientifically shut in to believe that there is a substance present in this latter case which is absent in the former? It is only trifling in such a matter to say "if we knew all the conditions," or to say "we can conceive of such and such things." The case is before us and in full comparison, as truly as the real and sulphurless gunpowder, and the inference in both cases is equally clear. It is of no use to say we "*imagine*" a spirit beyond the ganglions;—we imagine nothing,—we *infer* a spirit, as we infer the sulphur in the gunpowder that explodes. No one will say we "*imagine*" this sulphur,—why then should he say we "*imagine*" "*mind*"? I am informed by one of the first men in Britain as an experienced authority in mental maladies, that the brain of a man dying in perfect sanity has been compared with that of one dying in madness, and that by no means of which science is possessed, could there be detected the slightest difference between the nervous masses. Is there any inference in science more legitimate than that which would deduce from facts like this, the existence of a substance capable of derangement and distinct from the nervous matter in man? How is it that we conclude that certain substances are in certain combinations of substance? In no other way than by certain effects which show their presence. How do we conclude that a mental substance is present in that combination of substances to which we give the name of a living man? Just by modes of being indicated by their effects, and which belong to no material substance whatever. How does Dr. Davey know that there are ganglions in the human body but by effects which indicate their peculiar modes of existence? How do I know that there is such a thing as a mind in a man with whom I am acquainted? Exactly in the same way:—by the peculiar effects which that mind from its qualities produces not only upon my senses but also upon my thinking and emotional self. We thus reach the reality of mind as a substance just as we reach the reality of matter. We are forced, if we would not be stupidly ignorant, to know that there are two great classes of substances in the universe—two classes because essentially distinct in their

modes of being—the one which we call “matter” and the other we call “mind.”

It is now time to state, that matter never moves or changes itself. It is said to be inert, or in other words to exist in a mode of passivity. This is denied, and the opposite strongly asserted. It is required therefore that we should be careful to understand what we state, and also to verify the truth which is really stated, when we say that matter never moves or changes itself. True knowledge often depends more on a clear understanding of the truth, than on any evidence required to establish its verity.

I have no idea that there is a “*vis inertia*” in matter. A “*vis inertia*” is an absurdity. It is a “*forceless force*.” A piece of lead held in the hand apparently presses that hand downwards. The *force* which so presses downwards is as really force, as is that of the arm which bears the hand upwards. The question in such a case is, not as to a “*forceless force*,” but as to whether that force, which presses downwards, is the force of matter, or, like that which bears upwards, the force of mind. We shall learn more as to this question afterwards—at present I only state that *inertness in matter* does not mean a *force*, but the opposite of a force,—a passivity which requires a force in order to any change whatever taking place in this material substance. But we must explain still more fully.

When we say that matter is *inert* we do not mean that it cannot be put in motion. For example a stone thrown from the hand is matter put in motion. It continues to be in motion after it has ceased to be in contact with the hand. It is not on that account capable of moving itself. It is consequently *inert* or *passive*, as every one understands the word who really knows what it means. A mass of coal on fire is matter in a state of motion; for what is called combustion is only a state of complicated motion in the material which is burning. Certain materials brought together in a certain way enter into this state of combustion, just as the stone enters on its course through the air, when thrown from the hand. But that does not constitute these materials self-moving, any more than the motion of the stone disproves its *inertia* or passivity. As the stone is thrown from the hand, so the materials for combustion must be brought together by a similar agency. A rifle ball passes on its course with a very rapid motion, and with great force, in consequence of the combustion in the rifle barrel. That combustion is a consequence of the explosion of the cap on the nipple, that again of the snap of the hammer on the cap, and so on; but

no part of all this process shows that matter is self-changing or self-moving, inasmuch as the entire process amounts to nothing more than matter put in motion, and continuing in that state of motion till the impulse given it has been expended. The myriad facts that occur in the wide field of experimental chemical science, go all to prove that matter can be put in motion, but not one of these facts admits, I think, of the shadow of a doubt as to the great general truth, that what we universally call matter is incapable of self-change or self-originated motion. It is a grand mistake to think that this truth is in any way dependent on the biased conceptions of a theorising mind. We have no need here to say that we cannot conceive of this, or we can conceive of that, for we are not looking at abstractions but at experiments. Our belief is of that which actually occurs as distinguished from that which never does occur.

It is held, no doubt, by some that there is "latent force" in matter, by which it is somehow capable of moving and changing itself. If this is to be understood as meaning that matter is capable of being put in motion to such a degree that the motion will prove very forcible indeed, the idea amounts to no more than that a stone is capable of being thrown, and a rifle charge capable of being fired off. It does not even approach the subject of the *inertia* or essential *passivity* of matter. Any quality in a material substance which if first acted on by mind will issue in what is called *force*, is nothing more in careful thinking than that which belongs to a stone of the dullest kind. The spring, for example, which has been bent by a powerful arm, when set free has great force, but this is only like the motion of the stone after it has left the hand. That spring let alone would be just as *inert* and powerless as the stone when allowed to lie on the ground. The Leyden jar charged with electric force, as it may be called, may well enough be looked upon as full of bottled lightning itself, yet not only is that so-called force perfectly *inert* or *passive* till acted on by some mind, but it can be bottled up for use only by such a mind acting upon it.

We come here again, however, upon a theory in physics which, though I confess it is to my thinking of the wildest character, is to be carefully examined, because supported by the influence of the greatest names, and consequently very widely assumed. Its essential element is found in the idea that "force" is itself an entity, and not merely a mode of being in mind. It is not very difficult to understand how such an idea should be the result of a certain habit of thinking, though it is one thing to account for the idea, and

a very different thing to show, as must be done ere it is philosophically accepted, that it is a legitimate inference from fact. If any one is habituated exclusively to the contemplation of motions which he regards as "forces," in an incessant and protracted watching of these "forces" in their action, they may so occupy his thought, as to seem to him the only realities in the universe. The magnetic affection of a piece of iron, for example, called the magnetic force in that iron, may be exclusively thought of, till it seems to the exclusive thinker a substance as real as the iron. It is but a changeful mode of the iron's existence, which might pass away, and the substance be all there as before; but it ceases to be so in the exclusive thinker's state of mind, and becomes itself an entity—in fact becomes a thing while it is only a movement! The abstract idea of force, like the abstract idea of everything else, is nothing but a state of the thinking mind at the time when such an idea is entertained, and the idea of the force of any actual substance is only that of a state or mode in which that substance exists at the time when it has that force; but when anyone has given himself up to exclusive thought of the mere manner of a thing's existence, it soon becomes, as we have said, a thing itself to him. But we are not concerned so much here with the way in which the idea is formed, as with the *legitimacy* of the idea considered as an inference from the facts of nature.

There is what we think a very clear distinction which is of great importance in such investigations as that which we are now pursuing. It is that between force and motion. If we take such a machine, for instance, as that of Mr. Wyld, by means of which the French authorities, as well as our own, are endeavouring to furnish the light-houses along our coasts with the electric light, we have a good illustration of this distinction. This machine, when on a small scale, is driven by the hand—when on a larger scale, it is driven by a steam engine. By the turning of a crank a system of toothed wheels and pinions is set in motion—the motion of these is communicated to a part of the machine which revolves with great rapidity near the poles of a series of powerful magnets, collecting the magnetic currents from them. The ordinary motion is thus allied to the magnetic motion, which is changed into an electric motion, and concentrated in the poles of the machine itself. The result is a stream of electric motion which is almost incredibly powerful. When that stream is changed again into that peculiar movement which we call *light*, it is so strong as to make itself visible on the surface of the ocean at three or four times the distance at which the best lamps with

their reflectors have yet been able to send out their visible rays. If we begin with the immeasurably powerful light of this apparatus, and go back from this along the chain of movements, say till we have passed to the fire of the steam-engine by which the electro-magnetic machine is driven—if we are careful in our mode of inquiry as we go along—we shall find that we have not even the shadow of anything which can be philosophically called *force*. All is only motion. The light is but a state of movement in the atmosphere. The electricity is a similar state in the materials in which it is concentrated for the time. So is the magnetism—*only motion*. So, sure enough, is the motion of the machine from the crank onwards. So is the steam in the steam-engine. So is the heat and the combustion in the boiler and beneath it. So was the collection and the arrangement of the fuel, and the application of the match. So were the movements of the muscles of the person who made all ready. So were the cerebral changes, if you will, that produced the motion of these muscles. True science allows not a thought of anything in all this, but states of motion. There are motions that somehow give manifestation of a truly wonderful force, but, from the first to the last, not one of them, nor all of them put together, indicates that the force resides in them. There is something upon which the starting and the continuance of the whole chain depend. *That in truth*, and *that alone in the case*, is strictly and properly *FORCE*. It is not motion, but *that which puts in motion*. No thinking that is worthy of the name will overlook so obvious a distinction as this, nor can anyone who does overlook it, reasonably expect to reach anything but error as his conclusion, if he pursues such an inquiry as that with which we are here engaged. We shall see how the overlook leads to mistakes and confusion as we go on.

It is no doubt true that men who are held deservedly high in the world's estimation are responsible for the idea that force is matter, and that other first-rank men hold and teach that force is a separate entity which is neither matter nor mind. The notion that force is matter is, I humbly think, the culmination of that which represents force as other than a mode of being. I shall therefore attend to the latter idea before we enter upon the consideration of the former. But here we may remark that we need ever to keep in view that great discoverers of facts in nature are often the very worst reasoners in working out the ideas that are to be truthfully gathered from the facts which they discover. Every man seems to have his own department in which to be useful in promoting the advance of

human knowledge. One observes that which actually occurs, but he depends upon another, perhaps upon several others, for the true interpretation of that which he has observed.

One of the best essays on what is called "force" in nature is by Grove. He styles his subject the "*Correlation of Forces*," and shows, I think, with great conclusiveness of argument, that all the "affections" of matter (as he calls them) are only modes of that state of material substances which we usually call *motion*. What all understand as motion is (as we all know) a state of the moving object generally as a mass; but heat is the same and light also; only these are motions of the molecules or constituents of the mass. Electricity, magnetism, and "chemical affinity," or, as I should prefer to call it, chemical action, are only modes of agitation or motion in the matter affected. All the so-called "imponderables," such as "caloric," the electric "fluid," and the "ether," imagined as filling up the spaces between the atoms of matter, are thus disposed of as nonentities.

Grove speaks of the "*inertia*" of matter, but we are not sure what he means by the word, for he speaks of all matter as in a state of perpetual change. According to one mode or another every atom of the universe is regarded by him as in continual *motion*. He speaks of portions of matter as held in "equilibrium" by equal and opposing forces, and in this alone can we see anything that can be called *inertia* according to his view. So far, however, we find no great reason for adverse comment on his ideas. But when we endeavour to get hold of his idea of "force" he escapes us effectually. He says—"The dynamic theory regards heat as motion, and nothing else."* This is his own theory. So he regards heat as "motion and nothing else." But he says a little further on (page 69) that "We only know certain changes of matter, for which changes heat is a generic name; the *thing* heat is unknown." Then he goes on to say that "heat having been shown to be a force capable of producing motion, and motion to be capable of producing other modes of force, it necessarily follows that heat is capable, mediately, of producing them." We quote these words for the purpose chiefly of showing how loosely even such eminent men will reason. If heat is "motion and nothing else," how can it be "force?" Or, if heat is force capable of producing motion, how can it be that very motion and *nothing else*? Motion is a state of matter—*force is not*, as Grove's own words seem to show, but a property or an energy, by which, as he says, this state of motion

* Grove's *Correlation of Physical Forces*, page 66, edition 1862.

is produced. We must hold that it is nothing short of folly to reason as if that which agitates a body were the agitation which the agitating agent or force produces. Men apologize for such reasoning by saying that they cannot find suitable language in which to convey to the ordinary mind the new ideas which they have found; but the apology is worthless. Let them only have ideas that can be clearly *thought*, and they will soon get the right words by which those ideas can be correctly *expressed*. It is not the words "force" and "motion" with which we have any reason to find fault, but the idea of that which produces motion being motion itself. "Heat is motion and nothing else;" it is a state of matter in motion, and nothing more; the thing heat is unknown; yet this very heat is the force which produces this very motion—that is, heat produces itself! Not that some heat produces more heat, but that one heat produces that very heat!! The words are only too good, for they make the absurdity of the idea perfectly patent.

But there is invaluable instruction on this very subject to be gathered from Grove's teaching in his admirable essay. He gives as an illustration of the correlation of forces, a chain of changes, each link of which is only a peculiar mode of motion. He says—"At my lectures in 1843 I showed an experiment by which the production of all the other modes of force by light are exhibited. I may here shortly describe it:—A prepared Daguerrotype plate is enclosed in a box filled with water, having a glass shutter over it. Between this glass and the plate is a gridiron of silver wire; the plate is connected with one extremity of a galvanometer coil, and the gridiron of wire with one extremity of a Brequet's helix—an elegant instrument formed by a coil of two metals, the unequal expansion of which indicates slight changes of temperature—the other extremities of the galvanometer and helix are connected with a wire and the needles brought to zero. As soon as a beam of either daylight or oxyhydron light is, by raising the shutter, permitted to impinge upon the plate, the needles are deflected. Thus light being the initiatory force, we get *chemical action* on the plate, *electricity* circulating through the wires, *magnetism* in the coil, *heat* in the helix, and *motion* in the needles." He speaks of these successive changes in the state of the matter in hand "as modes of force," when all his reasoning goes to show that they are modes of *motion*, and, as he says in words already quoted, "nothing else." He speaks of light as the initiatory force, though he proves elsewhere that light is not a force at all, but a state of motion or agitation in the molecules of illuminated matter. But it is not with this that

we have chiefly to do at present, but with the positive truth that he is unwittingly teaching. His experiment demonstrates what is exactly to our purpose—namely, that the true initiatory force in the case is that which raises the shutter. This is not light, nor is it any other mode of mere motion; it is an energy which “produces motion.” Without this, *which is really a property of the mind of him who raises the shutter*, even the finely arranged instrument which Grove uses in his very interesting experiment* would be still for ever—that is, the matter which is under experiment, while it is capable of being put in motion by mind, is itself *inert* or utterly incapable of spontaneous change. Grove himself expresses this in one of his own statements. Speaking of an experiment, he says—“A voltaic combination is thus formed, and electricity, heat, light, magnetism, and motion produced at the will of the experimenter.” This “will of the experimenter,” or, as we should prefer to say, this experimenter himself, is transparently the true cause in the case, and in every case in which matter is the subject of experiment. True science, therefore, shuts us up to the great truth that mind alone is possessed of that force which is the true and efficient cause of motion or change in all its modes.

But we must return seriously to the notion that force is matter. The truly great names of Boscovich and Faraday are committed to this wild hypothesis. Faraday says—“Gravitation is a property of matter depending on a certain force, and it is this force which constitutes matter.” This statement expresses the conclusion at which he arrives by a somewhat elaborate argument given in the form of a letter addressed to Richard Taylor, Esq., and dated Jan. 25th, 1844. †

Mr. Faraday’s argument is partly metaphysical. He says, “A mind just entering on the subject may consider it difficult to think of the powers of matter independent of a separate something to be called *the matter*, but it is certainly far more difficult, and indeed impossible, to think of or imagine that *matter* independent of the powers.” This is merely the argument which we have already considered in proving the reality of substance, only it comes under notice in a peculiar phase. We have here to do with what are called “powers,” and two of these are specified in the course of the argument—the *conduction* and *isolation* of electricity. Shellac, for example,

* The ingenious arrangement of materials which form the instrument itself is also the work of the mind, intelligence, and will of its constructor.

† *Experimental Researches in Electricity*, Vol. II., page 293, edition 1844.

is said to have great isolating "*power*"—gold great conducting "*power*." It is held to be impossible to think of these substances apart from these "*powers*," but quite easy to think of such "*powers*" apart from these substances. The case may be just reversed, however, if we make sure of what we really mean by "*matter*," and what by "*powers*." What is called electricity is nothing, as Grove so strikingly teaches, but a peculiar state of motion among the particles of an electrified substance, just as heat is another peculiar state of motion in similar particles. Conduction is nothing but the passing of this peculiar movement from one portion of a material substance to another. Isolation is the arrestment of this peculiar motion so that it does not pass. The particles of shellac do not transmit the motion while the particles of gold do so. But this motion is only a state of these particles and the absence of the motion is equally a state. In themselves the motion and the stillness are absolutely nothing. They can be thought of, apart from that which is in motion, or which is still, only as nothing. As Mr. Grove says, "*the thing heat is unknown*," so certainly the *thing motion* is unknown; so is the *thing electricity*. This is not all. That state of the substance called shellac which is spoken of as the "*Power*" of isolation, and that state of the substance which is called gold which is called the "*Power*" of conduction, are just as little *things* as *heat*. The *things* isolation and conduction, apart from the substances isolated or electrified, are unknown. These states of things can never be things themselves. The difficulty of thinking of a substance apart from its states or qualities, is just the difficulty of thinking of the existence of an object apart from some mode of existence; but that difficulty does not necessitate our converting the mode of being into the being itself, nor of our converting the being into its mode of existence. Our thought of two involves our thought of one, so our thought of existence involves our thought of a mode of that existence. But, as the necessary thought involved in the thought of the two, is just as good a thought as that in which it is involved, so the thought of a mode of existence is just as good a thought as that of existence itself, and the thought of substance as good as that of quality, or mode of subsistence. Mr. Faraday is sadly misled in his thinking for want of perceiving these truths. For example he speaks of—"Molecules of something specially material, having powers attached in and around them,"—as if this were the idea of those who believe in the substantial existence of matter. Now, you can never speak of the motion of a wheel, for instance, as a power "attached to the wheel," or "gathered around it." It is a

mere state of the wheel at the time when it is in motion. Nor can you speak of the circular form of the wheel, in virtue of which it moves in the manner it does, as a power "attached in," or "gathered around" it. Neither is any other attribute, quality, property, capability, or mode of being belonging to the true idea of that wheel, a power attached to it or gathered around it. The wheel, if it exist at all, must exist as something, and so must have some mode of being, but this mere fashion of its existence is nothing, even in thought, apart from the thing of whose being it is the fashion. How then can the electric conduction or isolation of a substance, or that state of the substance which explains its isolation or its conduction be a "power attached in or around" that substance? Such thought, as places the modes of being as powers attached to or gathered around a substance, might pass in poetry perhaps, but is utterly from home in severe thinking. Yet it is only by taking mere states of substances, and imagining that these mere modes of existence are the substances whose modes of existence they are, that Mr. Faraday metaphysically reaches his amazing conclusion and teaches that "force is matter!" Force, as we have seen, and shall yet more fully see, is not even a mode of material existence, belonging as it does exclusively to mind, when considered in true science, and yet by this incredibly loose thinking it is made to seem matter itself! He says that with the view he opposes "a mass of matter consists of atoms and intervening space," but with the view which he adopts, "matter is everywhere present!" He constrains us to inquire what he means by "*where*." The word in relation to matter properly expresses the idea of *Place*. We can think of a place either as empty or full. An absolutely empty place is nothing. A place materially full is *in itself* equally nothing. If matter is everywhere present, it is infinitely *extended*. Matter is then the true *infinite*. This is, we should think, rather difficult of proof. If there is no empty space between its parts that is only that it is undivided if not indivisible—a *vacuum* is then impossible, which, we should think, is also rather incredible. It is certainly not an *unnatural* thought, that when a solid mass is moved to one side its *Place* is empty so far as this removal is concerned. Another mass, one would think, is required to take that place, or that must be empty,—that is, what is called "space" must there intervene. Matter is certainly not *necessarily* everywhere present.

But Mr. Faraday's argument is directed chiefly against certain aspects of what is called the "atomic" theory of matter, and against certain statements of this theory it may be

conclusive enough without touching the great question as to substantial existence. It is mainly a physical and not a metaphysical argument against the atomic idea. According to that hypothesis, matter is believed to consist in its "inner nature" of infinitesimal particles called atoms, which are themselves incapable of change, but which in their movements and combinations give the varied qualities to material objects which we observe them to possess. These atoms are regarded as not really touching one another, but moving each within a "*space*" surrounding it,—all being more or less distant from one another, according to the degree of molecular density in the matter which they compose. It is on a mistake in reference to this "*space*" that Mr. Faraday founds his argument. He says that, according to the atomic theory, "*space* must be the only continuous part in matter," for the particles are considered as separated by "*space*" from each other. Now, "*space*" in the absence of matter is just the opposite of continuity. Instead of being continuous at all, it is that which, in its essential emptiness, constitutes all breaks in continuity; so far, at least, as matter is concerned. Arguing as if space were matter, and the only continuous thing in material objects, he takes as one example a piece of shellac, which is an insulator for electric agitation, and says that, according to the atomic theory, the "*space*" between the particles of shellac must be an insulator. But, as Mr. Grove expresses it of heat, the *thing* space is unknown. Empty space is simply the absence of all substance—the idea expressed by the phrase is equivalent to that expressed by the word *nothing*. And this mere nothing, or absence of all substance, cannot, in the sense in which shellac is an insulator, be either an insulator or a conductor. The manifest truth in the case, whether we take one theory of matter or another, is that the particles of shellac are not in a state to move on the approach of the electric wave. They have somehow such a nature that they are like rocks in the agitated tide of electrified matter. It is *this* that constitutes shellac an insulator. This view of the case is, we humbly think, beyond all question, on the understanding that electricity is only a particular kind of motion in the molecules of conducting matter. Faraday, having laid down the mistake which we have thus indicated as his foundation, proceeds to take "*platinum or potassium*;" and, as these are conductors, he says that, according to the atomic theory, "*space*" in them must be a conductor! But there is no such "*must be*" in the case. The particles of platinum are moveable in the electric current, and so they readily move on the approach of that peculiar agitation. The space, hypo-

thetically imagined as existing between them, is nothing; and, *as nothing*, it can have nothing to do with the conduction of the electricity. The entire argument, therefore, of this truly great electrician goes for nothing, so far as his reasoning from continuity in "space" is concerned.

There is one part of the argument, however, which calls for a *separate notice*. He founds this upon the statement that "a space which can contain 2,800 atoms, and amongst them 700 of potassium itself, is found to be entirely filled by 430 atoms of potassium, as they exist in the ordinary state of that metal." On the ground of this statement he founds the conclusion that there must be far more space than matter in potassium; "yet it is an excellent conductor." So he says again, "space must be a conductor." But we say also again, that there is no such "*must be*" in the case. The necessity of truth is in the opposite direction. Space, which in the absence of a substance is nothing, cannot be either a conductor or a non-conductor. The particles of potassium pass into a state of electrical agitation on the sufficiently near approach of electrically agitated matter—just as the particles of water become agitated when the wind or tide approaches, and the particles of the massive rock are still—that is the fact as demonstrated by experiment—the space in which they are agitated having neither more nor less to do with the conduction than the space in which the experimenter himself moves has to do with his movements.

There is, however, another notable idea in this remarkable letter of Faraday. He shows that "the volume, which will contain 430 atoms of potassium, and nothing else, while in the state of the metal, will, when that potassium is converted into nitre, contain very nearly the same number of atoms of potassium, *i. e.*, 416, and also then seven times as many, or 2,912 atoms of nitrogen and oxygen besides." He gives another instance of the same thing in another substance, and refers to many others, all proving that a vast number of atoms may and do occupy the space which seems full with comparatively few. He is foreshadowing in these statements that in which the wildness of his speculation is most effectually seen—his denial of the mutual impenetrability of matter. His theory, after Boscovich, is that atoms are not particles of mutually impenetrable substance, but "centres of force," to which centres there is neither length, breadth, nor thickness! They are merely "mathematical points," and need *no space* for their accommodation! Space according to this idea is not even a requirement of material existence! Strictly these centres of force are *nowhere*! That which occupies no *space* is simply

in no place—that is, as I have said, *nowhere*! I do not remember ever seeing such a strange contradiction of ideas solemnly stated as philosophy as we find in this part of the letter. For example, he says, “Doubtless the centres of force vary in their distance one from another, but that which is truly the matter of one atom touches the matter of its neighbours;” and yet he says that, according to his assumption, “matter and the atoms of matter would be mutually penetrable.” The impenetrability of matter simply means that two parts of it cannot be in one and the same place at the same time. According to this theory of Mr. Faraday, not only two particles of this matter, but any number of them, can be in one and the same place at the same time, and that though they vary in their “distances” one from the other! We think that nothing can be more certain than that, if the least particle of matter can be in the same place with another particle, and hence any number of particles occupy that same place also at that same time, the masses which the accumulation of these particles form must be just as mutually penetrable as their minutest parts. The mere multiplication of that which needs *no* space for its presence can never call for space, and hence the infinitely absurd conclusion that the earth itself needs no space in which to revolve! But what does this really mean? It simply means, when analyzed, that extension is not a mode of existence in material substances! Space and extension are identical in true thinking, unless you regard space as a mere possibility of extended existence. This is the true notion of the idea in the abstract; and, if substance does not need space, it has not extension! Here we fall back on our idea of knowledge as a legitimate inference from the revelations of consciousness, and ask whether it is such an inference, that material substances have no size? Is it a mistake to imagine that certain objects are really long or short, or deep or high? It will certainly require very powerful chemical experiments to convince us that mankind are mistaken in believing in extension as a mode of material existence. What are the experiments of this most eminent among electricians? He says, “as regards the mutual penetrability of the atoms, one would think that the facts respecting potassium and its compounds, already described, would be enough to prove that point to a mind which accepts a fact for a fact, and is not obstructed in its judgments by preconceived notions.” But what is the fact? Merely that 3,328 atoms will go into a space which seems full with 430! It would be about as powerful reasoning to argue that because a carpet-bag, which one person says is *crammed*, will take in three

times as much again, therefore trousers, boots, stockings, &c., &c., are mutually penetrable; and, having nothing of the nature of size, can exist in the same space at the same time in any quantity! We accept the fact as to the packing of atoms in the potassium as we accept that of the packing of clothes in the bag, but the *conclusion drawn from the fact*—that is the only thing in question, and it *defies belief*. Yet this is the reasoning on which we are called to have the assurance that “force is matter.” He must advance something very different from this wild creation of the untethered fancy, who can rightly claim to set those down as under the domain of prejudice who refuse his theory. We come back, therefore, to our first statement on the point, and abide by the *inertia* or utter *passivity* of matter.

It is at this point in our inquiry that we are prepared for the statement that *mind alone is cause*. Mind is cause in that sense that it originates change; mind alone is true cause, inasmuch as it alone originates motion or any true change, either in itself or in matter. It is, as metaphysicians say, the only “*efficient*” cause.

Here we are met with a flat denial of the statement thus made. John Stuart Mill says: “To my apprehension, a volition is not an efficient, but simply a physical, cause. Our will causes our bodily actions in the same sense, and in no other, in which cold causes ice, or a spark causes an explosion of gunpowder. The volition, a state of our mind, is the antecedent; the motion of our limbs in conformity with the volition, is the consequent.”* Such is the doctrine in defence of which Mr. Mill argues as if the whole thing must turn on what we can or cannot find for the present in “his apprehension.” The question is to be settled by legitimate inference from the facts to which it is related. It may be settled without reference to the “apprehension” of any one, by a careful examination of men and things as they stand in what is called the external world. First of all, a *volition* is nothing apart from a person whose volition it is. Will is nothing but a mode of being in a person who is endowed with will. Will is only a capability of volition as explosiveness (to recur to one of our illustrations) is a capability of gunpowder. Will, however, is the capability of a *person*, and explosiveness the capability of a *thing*.

The essential distinction marked by the words “person” and “thing” is not that merely between the conscious and the unconscious, though that is a most important distinction. It

* *Logic*, Vol. I., page 390.

is as truly that the person *originates* change, while the thing *never* does so. Now there is no induction, as already remarked, more perfect than that which leads us to *mind* as the originator of motion or change. Take Mr. Grove's beautiful experiment, already described, showing the correlation of forces—begin at the last and proceed to the first in the series of changes, and you reach the person who "*at will*" raises the shutter, or allows it to remain closed. You may *imagine* that the volition of this person is the result of a change in the brain, preceding it, but you do not need to imagine any other link in the chain. The movement of the needle is a visible fact—the heating of the helix is a fact—the magnetism of the coil is a fact—the electric motion of the wire is a fact—the chemical change is a fact—the admission of light is a fact—the raising of the shutter is a fact—the motion of the finger is a fact—so is the act of will a fact. But where is the evidence of a material change going before this act of will in the person who raises the shutter? We have nothing to do with Mr. Mill's "apprehension," or with the apprehension of anybody else. We have to do with facts that are palpable to all who choose to look at them. It is only trifling to talk of what one can conceive and another cannot conceive, in a case where the plainest and most unquestionable matters of fact compel all alike to come to one conclusion, or to escape into the region of mere "apprehension" for argument by which to oppose these facts. If matter were capable of originating its own changes it would surely be possible to find at least one instance in which it has been found to do so. But, so far as man can by experiment question this substance, no change, or series of changes has ever been discovered in which a mind, or in other words a *person*, was not at the origin as the first mover. To refer to the changes that go on in nature would be simply to beg the question, should any one say that these are originated by no one. For, when in every case in which it is possible for man to test the nature of material objects, they are found *inert* till moved into change by a person; on what ground can it be proved that they cease to be *inert* when beyond the reach of man?

Mr. Mill speaks of its being more congruous to our natural conceptions to believe that matter acts on mind, than to believe that mind acts on matter. It is of very small moment in a scientific question, what may happen to seem congruous to a man's conceptions. We must look at the facts, and not at our conceptions apart from them. In every case in which we have true access to a chain of facts in the material world, there is a first link beyond which we cannot come. It is that first fact to which special attention needs to be called in every

inquiry as to *cause*. Here, for example, is a timepiece, and we may begin with the pointers, which at this moment have reached the position in which they mark the hour of noon and indicate accordingly. We follow back the motion which has so placed them from wheel to pinion, and from pinion to spring or weight, as the case may be. But, if we follow on, we at length reach the person who wound up the machine. So far as the first fact in the clock's motions is concerned, we reach the mind of that person, but can go no further. The volition, or act of will, on the part of that person is the first fact, and *never in any case* does a movement of matter occupy that first place in such a chain of motions. If we meant nothing more than the first substance to move in every chain of such movements, when we speak of *efficient cause* we should be compelled in true science to assert that mind alone is that cause. To speak of an "assemblage of conditions" as the cause of any effect, may suit for an explanation of language, which has been excessively loosely used and greatly needs explanation; but when we are not in search of an explanation of loosely employed language, but are seeking for the truth itself, we must fix the mind on that which begins the series of changes whose cause we are desirous to know; and as we do so, we find that in every case in which we can reach the first motion in the chain, we land in mind, and are therefore compelled to believe that *mind* was the first mover in the chain, and that mind *alone* is cause.

It is no doubt denied that we have any positive evidence to prove that mind possesses causative energy. It is not easy to know what is understood by such "positive evidence." If a rifle-bullet is seen to pass through a good-sized plank, we imagine that most minds in a state of sanity would accept that fact as positive evidence that there is force or causative energy somewhere in connection with the occurrence. But, if we trace back the chain of motions from which we are able to know that this motion through the plank originally sprang, and if we find that the whole chain would have been non-existent but for the mind that willed to draw the trigger, we should think we have something very like "positive evidence" that causative energy is a property of that mind. You may call that something by which the impulse is originated any sort of name you choose, but it is there in reality as something utterly different from all that merely proves the medium of transmission to the impulse, or movement. It is that which moves, or, at least, is the first to move, as distinguished from that which is moved, or only follows in the wake of the first mover, and it is invariably *mind—never matter*.

It may be well here to consider more fully the reality and true nature of volition. Is there such a thing as true *will*? In other words, is a man conscious of the capability of truly *originating* a line of motion by being the first to move in that line? As an illustration of our question, suppose a chain of changes, such as Grove places before us in his beautiful experiment already described, and in which he says "light is the initiatory force," but in which he shows that "the will of the experimenter" who raises the shutter is really that force. Is this *will* a myth, or is it a real property of the conscious mind? It is admitted that, as regards external objects, we know our sensations. We, then, do at least know our sensations. But do we know these sensations in any manner in which we do not know our volitions? Is not our knowledge in both cases equally immediate and necessary? If I have the sensation of cold, and you insist that I do not infer that I am cold, but that the sensation is matter of direct consciousness, then, if I will to raise my arm, do I any more infer that I *will* than I infer in the former case that I am cold? I am disposed to think that I infer in both cases; but assuredly I am conscious of the one thing as directly as I am conscious of the other. Sensational consciousness is not more real than volitional consciousness; hence, if we may say that we know our sensations as feelings, we may just as truly say we know our acts of will as volitions.

But what is that property of mind with which we are thus as certainly acquainted as we are with our capability of sensation? In our sensations we learn of something without us which produces a certain effect within us. In our volitions we learn of something within us which is followed by effects that lie without us. The "*I feel*" expresses the former; the "*I will*" expresses the latter. Take the case of the scientific experimenter as our illustration again. His instrument, we shall say, is all arranged and ready for action. He sees it—that is sensation; but the instrument is motionless. He feels it merely—that is sensation; but it is yet motionless. So long as he has only sensations from it, the experiment refrains from beginning. All is ready, including his own material organization, which is as necessary to the changes to be effected as any part of the machine, but there is no experiment until he moves in an act of *will*; then the shutter is raised, and all the motions follow. You may just as philosophically say that he knows nothing at all, as say that he does not know of this causative act of his own mind. Then this act is essentially different from all mere effects produced in the mind, such as sensations. It is not part of our consciousness

in the case of any sensation that it might be otherwise than it is, and that in the same circumstances. If touched with a red-hot iron, no one conceives that he may or may not have a sensation of heat; or, when he has that sensation, thinks that it might be just the opposite if he pleased. But every one knows, in a true instance of volition, that he may or may not will, and that he may will otherwise than he does. Even, then, if we admit that we know only of antecedents and consequents, it remains irresistibly evident that the first mover in every series of changes that lies fully within the reach of human observation is the mind in its act of will. But this moving of *mind*, which is the first antecedent, is essentially unlike all mere consequents. It differs from all sensation, not only as one sensation differs from another, but in the very characteristic by which a cause, properly speaking, differs from an effect.

Here, however, we are met by something like the assertion already alluded to, that this movement of mind which we call willing, or volition, is itself only a consequence of material movements. Those who imagine that the only *cause* of which we can properly speak in discoursing of natural objects is an "assemblage of conditions," are strongly tempted to look at the mere "assemblage of conditions" which precedes an act of will as the cause of that act. It is well to keep in mind that, even were this true, it would not in the least degree alter the fact that, in all those chains of material change which we can fairly test by experiment, mind is the first mover. We are, however, led by this notion—that volition is itself only an *effect*—into a totally different field of thought from that in which we observe the facts of the material universe. Our inquiry here is as to the nature of *mind*, not as moving *first* in a chain of otherwise material movements, but as moving *last* in such a chain. We all know that we have abundance of experiments in which the various modes of material movement *follow* the one spiritual movement of will. Here we must call for experiments in which this movement of will forms the closing link, so to speak, in a chain of material motions. A superficial thinker will probably conclude that these are very numerous. He will naturally turn to those cases in which painful material changes issue in volition. He might add to these, however, all cases in which pleasurable changes affect the volitional being. This is not his proper field of testing fact. He must be brought to deal with those other cases in which what may be called the *inertia of mind* is most signally manifest. The "will nots" must be carefully studied as well as the "wills." In the study of these, we think, he will

hardly fail to see that *there is* an element in what we have called the *inertia* of mind which *is not* an element in the *inertia* of matter. The amount of force necessary to move any portion of matter can be mathematically ascertained. To the infinitesimal fraction of an atom's weight (if we may use a hyperbolical and yet truthful mode of expression) force is *calculable* so far as the moving of matter is concerned. Will any man say the same of volition as a movement of mind? If he do, he is bound to prove his affirmation. If he could do so, he would prove that the universal blame which man attaches to wicked volitions is absurd and wrong, and he who opposes his assertion to the universal verdict—or to all but the universal verdict—of intelligence is bound to establish his position by irrefragable evidence, or to surrender it. He must take those myriad cases in which the most powerful and concentrated of all ascertained assemblages of conditions have failed to produce the "*I will*" of the fully determined mind, and he must show what condition, or degree of a condition, was wanting so as to account for the unchanging "*will not*" of the hero, or of the incorrigible. This is a case in which we must respect the truth, that the "*I can conceive*" of the philosopher goes for nothing. It is not one in which a "*may be*" can be accepted for a moment. The "*conceivability*" and the "*inconceivability*," together with the "*may be*" and the "*cannot be*," are not very important in any case of true science, but in this case they can have no place except as indications of something very like perversity. A mass of iron, for example, like the war-ship *Northumberland*, lies dead on the "*ways*." It is known beyond the shadow of a doubt that the amount of force necessary to raise and push her off into the river is mathematically calculated to the hundred-thousandth part of an ounce. This is demonstrable by endless experiments. But we deny that *one* experiment can be mentioned in which the force necessary to produce a volition in a mind is so calculable, and that, because in the case of mind there is the element of that which we choose to call cause itself—not in the sense of "*an assemblage of conditions*," but in the true sense of a producing power, so far as human action goes, as real as that of God Himself. It is the fundamental feature of His own image, as that is found impressed on men. When mind is really studied, as matter is really studied, not in dreamy *conceivings*, but by actual observation of facts, and the careful generalization of their teaching, it is placed beyond all doubt that mind is *cause*, and that this causative faculty belongs to mind *alone*. When we consider the general truth—the result of all the facts that bear on the subject—that

assemblages of conditions, which are in one instance followed by one volition, are in other cases followed by its opposite. It is this which constitutes the incalculable uncertainty of all moral influences, as distinguished from all physical influences. What is the true explanation? Simply that material motions are strictly mechanical, moral movements are not mechanical; that is, *motions* pass into one another in matter *necessarily*, but motions do not so pass into one another in mind. The man, in that which constitutes his manhood in its most essential element, is capable of arresting all movement when it reaches his capability of will, just as he is capable of passing onward, and of originating movement both in mind and in matter too. No careful reckoning of the facts of human experience and observation can miss those in which the man is thus a *first cause* of his own actions.

We are fully aware that men who are (within a certain limit) great in science hold that true causation is found in the will of God alone. Grove, from whom we have already quoted so much, closes his essay with these words—"Causation is the will, creation the act, of God." Such language is but the eloquent utterance of a mistaken idea. The evil result which we trace to a guilty man can no more be traced beyond that man's will in true science, than the act of creation itself can be traced beyond the will of the Creator. The "will of the experimenter," as Grove himself expresses it, is just as real, and just as really the first cause of the succession of changes which occur in the experiment, as is the will of God the first cause of the succession of changes of which he is the author. This is no matter of theory, or of so-called Psychology, but of simple induction, in which the facts guide us infallibly to their result. Take the man who deliberately raises his arm and murders his fellow. You trace all the sad consequences of his *volition* to himself, and you can trace them no further. No "assemblage of conditions" that ever occurred in the universe will account for that act apart from that first motion of mind which we call the volition, or act of will, in that murderer. To be a creator of worlds implies powers by which will may be carried out into the result, creation, which are not implied in the case of the murderer; but powers that are necessary to carry out will to its issue are distinct enough from will itself, and that will in both cases is the same capability of mind. It is not only unphilosophical, but mischievous in the extreme, to hide the real responsibility of man behind the error that causation belongs to God alone. It would be just as good sense to say that *mind* belongs to God only, as that efficient

mind is, in every case coming under our observation, the *first* to move, and that matter never is in any such case the first—this of itself is sufficient to suggest that there is something in this first mover, which is not in any of those that are moved, and in their turn move the others. Even if we could not in any way tell what this difference is, so as to give it an intelligible name, it would be wilful blindness in us to deny that there is a difference. If, as is manifest, no assemblage of conditions in which this moving mind is absent is ever followed by any change, so far as we can interrogate nature on the subject, we are shut up to regard this mind as having something in the nature of a capability of moving as distinguished from that of merely being moved. By fair induction we thus reach the general truth, that a man is the first cause of his own actions, and so the real and responsible author of all the consequents that flow from them.

The subject of "*motives*" comes naturally before us here. Materialists take great advantage of the false notions of their opponents on this point. A "*motive*" is that which *moves*. If something which *necessarily moves the man* in his act of will really exists in every case of volition, then the man is not the *first* to move. But does any such thing as this necessary mover of the man really exist? If it does so, it must be demonstrable. What sort of thing may it be? It must be either a substance, or a state of a substance. No one will contend that a "*motive*" is the former, so it must be the latter. A motive then is a state of a substance, and that substance must be either body or mind. As we have seen, states of the body are followed by acts of will; so are states of the mind. If our induction could be so lame as to be satisfied with this mere antecedence and consequence, then we might set down these states of body and mind as the *movers*, or as the true causes of volition. But by such an induction we might regard night as the true cause of day, inasmuch as night is an antecedent, and day its consequent. Our induction must be full. It must take in at least all *classes* of facts that bear on the point in hand. When we do take in all classes of such facts, we find that so-called "*motives*" as often fail to be followed by volition, as prove to be followed by it. If a motive is that which moves, what then is that which does *not* move? Or, if a motive is that which is followed by motion, what is that which is *not* followed by motion? It is *not a motive*. It cannot, to say the least, be that which *necessarily* moves. But the same states of body and mind that are in one case followed by volition, are in other cases followed by no volition. The same

causation belongs only to Him. While mind alone is cause, mind everywhere is cause in so far as it is truly mind. The immense importance of this truth will be seen when we come, as we shall soon do, to apply these principles which we are thus working out to the Christian doctrine of prayer. Losing sight of the fundamental idea of true will in man as well as in God, produces the most disastrous confusion in all that relates to a thorough religion, and in no department more than in that in which we have to do with supplication.

We are now prepared for the statement that mind has power to move and change that which is material; and here again we repudiate the test of congruity or incongruity with what are called "our natural conceptions." To one's "natural conceptions," as he chooses to call his mere ordinary notions, or habits of thought, it is congruous that matter should rule over mind,—to another's habits of thought it is congruous that mind should rule over matter. Such things ought never to be intruded as arguments into science of any kind. When acting scientifically we look for what *is*—not for that which may most easily be conceived. We endeavour to infer legitimately from the field of fact all that may be so inferred. Nor do we look vaguely on that field of fact, but take up the individual occurrences, scrutinizing each in turn, and gathering the general truth from a comparison of the whole so far as thus scrutinized. Say that we are desirous to know the true cause of the great tidal waves that sweep over the surface of the ocean. We do not look vaguely at that ocean, nor loosely reason by looking at individual tides on any particular part of a coast, nor do we look even at particular waves that follow each other, making by inches or losing by inches on the strand. We begin with a portion perhaps of seawater and experiment till we have a somewhat clear idea of its nature. It is fluid—that is, it can be made to *flow*—but is utterly incapable of spontaneous movement. We then legitimately infer that the ocean is not to have the tides ascribed to *itself* as their cause. We must look for that cause elsewhere. If it is not a tide of seawater whose movements we would explain, but a shoal of fishes coming along like a sea of life, and we are desirous to know the immediate or efficient cause of their progress, we take the individual fish and soon find its capability of spontaneous movement. We legitimately infer that this vast shoal is the cause of its own movement. We may look for *conditions* of that movement there, or for its "antecedents" if you will, but not for its cause. We have found that, which in the case of the passive fluid of the ocean, we had not found. As we rise in the scale of

life, this power of self-motion, and through that the power of moving and changing merely material objects, becomes more and more evident.

When we consider the extent to which man changes the material world from the most gigantic of his works to the most minute of his experiments in the laboratory itself, there can be no truth more evident than that mind moves and changes matter—even that frail mind which constitutes the man. It is no drawback to this argument to say that matter resists and often overwhelms man, because that proves only that man's power to move and change matter is *limited*. It tells us of a measure to the power, but no one will imagine that the measure of a thing annihilates the thing itself. Finding that in the human, and even in the animal sphere, the living spirit moves and changes matter; and that with man matter is to so great an extent at his will as Grove says, we are irresistibly led up to the infinitely greater mind in God, at Whose rule its movements and changes must lie infinitely more fully than they are at the will of man. It is not easy to look at a piece of matter and say what change man may not make on it. But when such is the case with the incalculably inferior mind, who shall rationally say what are, and what are not, the possibilities of movement and change in matter which lie at the will of the Infinite One? If we trace the history of human discovery as to matter, we find ourselves in a region of facts in which we constantly seem to be about to reach a limit beyond which human dominion over matter can go no farther, but the horizon is constantly receding. The more we discover the more wide the possibilities seem to be of future discovery. Who shall say what even man may not yet do, in the way of adapting the material universe to himself and to his happiness? But all that he can ever do will be necessarily only an infinitesimal part of what *that mind* can do, to whose originating *fiat* we are compelled to trace the very being of the universe; and this we are compelled to do from the moment when we infer that matter cannot move or change, far less *create itself*. When we have got thus far we have made a great step in the philosophy of prayer. We are now in that field of control within which He is a free and Almighty agent who is requested to act in all cases of true prayer for such things as involve material changes. Here, however, we only glance at that which will appear more fully afterwards.

It is at this point that we come upon the very important subject of "natural law." When we see clearly that mind is efficient cause, and that all minds are such causes, we occupy

a position in which this subject appears in its true limitations. So long as we know only one thing following another—what is called “antecedence and consequence”—in natural changes, we are fully exposed to the notion of an inexorable sameness in those changes. The knowledge of *true will* at once modifies this notion. You may calculate in a given case how water will run, and even how the wind will blow, but who can calculate in any case how a free will shall *decide*? He reckons without his host who studies the so-called “uniformities of nature,” forgetting that the material universe is constantly affected in what are to us its most important changes by moral agencies. Yet this is just how too many reckon, and hence come to fancy a world which is full of variations arising from both human and divine actions, as if it were a mere machine in which no one wheel could ever move except in one direction, and at one unalterable speed. Law represents only the idea of a generalized mode of action. All reasoning on “laws” which is confined to mere order of occurrences, is reasoning on the surface of things. It is like reasoning on the movements of a locomotive, and calculating on a certain speed for the train, forgetting the driver. I have known such a train leaving one of our most important stations and the chief man on the engine so tipsy, that the stoker threw him among the coals, and took his place, going off with the train alone. What if the stoker had been anything but steady? I have known a fine steamer leave one of our harbours and the captain unable to see from the stern to the bow of his vessel. He compelled his men to hold on with full steam till the ship was hard and fast in the mud of the opposite coast! Shipowners have something more to think of than the “antecedence and consequence” of material change. So has the true philosopher. He must see that the freedom of the actors who affect Nature, is as real as the laws according to which material objects are affected. In perfect accordance with the law of gravitation for example, I may raise a weight from the ground, or let it remain at rest, or push it along without raising it. It is not possible to take in the facts of the case as they ever crowd themselves upon us, and yet believe that natural law is anything else than the generalized mode of action on the part of those agents by whom what is called Nature is affected. If you *choose* to look at occurrences only and to ignore actors, you see nothing else but that to which you confine your view; but such limitation of vision is the opposite of rational.

When we fairly enter on the region of fact we find that the idea of an invariable order of succession in nature is only partially true, and, when applied universally, exceedingly deceptive.

He who founds his "inductive logic" on the notion of such an invariable order of succession, is adrift without rudder or compass the moment he leaves the region of inorganic changes. If he live among gases and such simple substances, and observe nothing but the laws according to which they are combined and dissolved when treated in given ways, he will work away tolerably with his defective reasoning; but he must not venture beyond the *inorganic line*. He will find that one part of hydrogen will always combine with eight parts of oxygen, when treated in the proper way for their combination, and that the result will be water. So long as he confines his investigations to such elemental matter his so-called "*law of causation*," as that of invariable succession, will suit; but when he begins to examine the lowest forms that have life, his "*law*" will fail him. Those antecedents whose consequent is a *lichen* or a *sponge* are not invariably followed by a perfectly similar result. One part of hydrogen combining with eight parts of oxygen always issues in water, and in water which is perfectly the same as any other water so formed; but whatever be the nature of that which gives rise even to a *lichen* it introduces *variation* the moment it acts. So strikingly true is this, that men of the most extensive materialistic science have been impressed with the *variableness* of succession in nature, till they are not indisposed to believe that the *lichen* itself may have developed in the course of myriads of ages so that its offspring is found at last to be a *man*! You thus find a votary of science at one time founding his whole fabric of reasoning on an "*invariable succession* in nature," and at another arguing as if the succession had been so *variable* as to account for the production, from some absolutely simple antecedent, of all the measureless variety of the universe! These are the results of that strange fancy, that so possesses us all at times, and in the indulgence of which we refuse to see with more than the half or even the tenth of an eye! We place two pure gases in certain proportions together, and do what is necessary to their combining chemically—the result is the same as it ever has been if the same experiment has been repeated millions of millions of times. But we put a seed into the soil, and from the germ we have a plant strikingly different from that on which the seed grew—strikingly different from those produced by the seeds that grew along with it in the same pod, resulting from the fructifying of the same flower; and all the plants from these seeds will give more or less variety from their seeds in their turn. The astonishing individuality of every living being, whether plant or animal, is dependent on this variableness of succession in nature. A man may as well deny that indi-

viduality, as assert that the order of nature is that of an invariable succession of events.

It can make no reasonable impression against this truth to say that "if we only knew all the antecedents" of any consequent we should find that it had occurred according to an invariable order of succession. This is but a begging of the question, and that in the most beggarly way—of insisting on a conclusion in the face of myriads of contradicting facts. If the same antecedents had always been followed by the same consequents, progress from the most simple to the most complicated forms of being would have been utterly impossible. As really as water is always formed when one part of hydrogen and eight of oxygen combine, so would the same results have always followed the same antecedents, and one invariable round must have been the only history of nature. But the indisputable facts of science, especially of geological science, demonstrate that this has not been the case. Variety of result has been the great law of life. Invariableness has been that of inorganic changes exclusively, and that is shown us only when we confine our attention to purely inorganic movements.

When, therefore, we are told that the changes in the natural world take place according to an invariable order of succession, and that this is the fixed law of nature, we are told what is *transparently untrue*. If such a statement is made in the name of scientific culture, it is made by one who is himself ignorant of some of the most irresistible conclusions of science, or who is oblivious to that very "law of variation" of which scientific men of the first class have tried to make so much. Such an invariable order of succession in nature, when brought to bear against prayer and its answer by God, is nothing but a frail fallacy, paraded in the face of eternal truth. The claim to "culture," to science, or to philosophy, which is associated with this folly, is a claim which is seriously deteriorated by that with which it is thus allied.

Here we come naturally upon that part of our wide subject where we distinguish in a more careful manner between that in which results are uniform and that in which they are not so. In the strictly material region effects occur in chains, so to speak. The creation of a first link is never a solitary occurrence. It involves other occurrences that are evolved in succession when the first takes place. Material objects are so connected that it is impossible to move one without also moving others as a consequence of that movement. In mind, considered in its capability of *will*, the case is otherwise. Everything may be moved round about that mind in its volitional capability, and yet that will may be *still*. This is

not a mere logical deduction from fancied premises, but the resistless teaching of fact. We all know, as we said before, that we can calculate with the precision and certainty of mathematics on the sequences of those purely material motions that follow an act of will, but we also know by abundant experience how impossible it is to calculate on that will itself. One of the fundamental truths of human procedure throughout its whole history is found in the freedom of man as a being capable of will; and that truth is more thoroughly proved by the variety of moral results, than the absence of such freedom in matter is proved by the uniformity of material results. But this constrains us to see that in a world in which there are millions of minds, each capable of true will, and where each within its sphere of volition is perfectly free, there cannot but be an endless variety and uncertainty of result. It is surely, then, anything but scientific to observe the results of material change alone, and to ignore the *causings of mind*. Such procedure can lead only to error. The men who are so anxious to assure us that "everything in nature is uniform," are also the very men who say to us, "if you will only live according to nature;" and they constrain us to estimate that "*if*" which they so constantly use. They force us to think of the truth which is implied in the "*if*"—the truth that we do not live according to nature—that truth also involved in that *if*, which is, that we *may* so live, and we *may not*; which again involves the fact of *will*—the fact of the existence of the most uncertain thing in the universe, or even conceivable. It is childish, then, to talk of a "uniform succession of events" in a world in which these millions of minds, or "*wills*," as they are so often called, are constantly demonstrating their freedom and their fickleness. You may think of a train of material changes which is ever so extended; if these changes are to occur, you must have a person who shall put the train in motion, and you may have many persons who will affect it when it is in motion. There lies the uncertainty. On what line of "uniform succession" shall we calculate in such cases? There is no such "uniform succession." Myriads of instances can easily be given to demonstrate the uniformity of mere material and inorganic chains of effects; but, as we have already said, not one instance to prove that the same uniformity belongs to the action of mind in volition. This clears our atmosphere of thought: we see where the uniformity lies, and we see, too, where it is absent. So far as changes are purely material, there is uniformity; but so far as they are the effects of will, they are not so. This is not the teaching of some fine-spun thread of logic, nor the voice in a philosophic dream, but the

"*legitimate inference*" from the facts of the case. And a most pregnant inference it is. For, in view of it, we see that His actings Who is the Great Cause must be varied to meet in true wisdom all the varied actings of created minds, so that in the fresh circumstances perpetually arising, the best that is possible may be ever done.

It is time now to look out beyond the world of merely created minds and things. We have so far anticipated this; but the change of view must be made deliberately and with great care. As we rise from minds that are limited to that mind which alone is infinite, and from those who are imperfect to Him Who is perfect in the fullest sense, we are beset with hosts of metaphysical bewilderings. We are told that we "cannot know," and yet it is made to appear as if we cannot help knowing. It is said that we *cannot reason*, but we *must believe*! This is not satisfactory to our thinking, so we must try whether reasoning is impossible, as we are told.

There is perhaps no region of thought that more urgently requires reforming than that in which we meet with what men call "the Infinite and the Absolute." Sir William Hamilton was one of the most influential of all mystifiers in this region, and he has been followed by a disciple who carries his mystifications to an amazing degree of perfection. We cannot help believing that a world of good must spring from any thorough change in this branch of speculation. John Stuart Mill, with all his faults, has done good service here.* Saisset has done yet nobler work in the same direction.† The change wanted seems greatly to consist in a fair distinction between infinity as an overstrained idea, and infinity as a mode of being in one who is properly the Infinite. "The Finite" abstractly is *nothing*. "The Infinite" in the abstract is just as truly *nothing*. A finite person or thing is that which is limited in its mode of being. An infinite person or thing is that which in one or more modes of its being is unlimited. The Omnipotent is unlimited in power; the Omniscient is unlimited in knowledge. But these ideas of infinity do not come up to the ideal—we might say *the idol*—of certain philosophers. They insist that we *must believe* in such an "Absolute and Infinite" as is "the complement of the relative and the finite"—that is, in *such an absolute as has no relations, and such an infinite as suffers no distinctions*! I am not at all sure as to those so-called "*necessary beliefs*." They remind us of a case in which the

* See his *Examination of Sir W. Hamilton's Philosophy*, pages 42 to 56. Edition 1865.

† See his *Modern Pantheism*, Vol. II., pages 46 to 76, Ed. 1863.

Duke of Wellington was entreated to send home a young officer, because his intended wife *must die* if he was not brought to her. The great soldier most reluctantly declined, but kindly hinted that such illnesses did not generally prove fatal. There are *fancied necessities* in philosophy as well as in love. I think this absurd idea of the Infinite is one of them. May we not deny "absolute" infinity intelligently? May we not imagine that beyond a certain range in the universe there is nothing? Can we not even *think* this? I insist that I can. I can think of a perfect *vacuum*, and that is nothing. You say it is "space;" but it is empty space, and that is *nothing*. It may be truly said to be the possibility of being, but that is not being itself. Where nothing is, something may be; but the nothing is a perfectly good thought. I must believe that the thought of a perfect *vacuum* is as good as any other idea. As easily as I can think of a *vacuum* in a perfectly-exhausted receiver, I can think of a *vacuum* beyond certain limits of the universe. A certain writer has said that if he were on the verge of supposed finite being he could thrust out his arm beyond, and so there must be something into which his arm could be thrust. We may improve on his illustration. If he stood on the edge of being, with only empty space beyond, he might leap into it, and there would then be a live philosopher where there was nothing before; but that would fail to prove the *being of that nothing*. I do not for a moment deny the true Infinite, but I do deny that the Absolute Infinite is a *necessary* idea. It is perfectly easy to conceive of the absence of being from what is called a *place*. The conception is perfectly clear, and just as satisfactory as any true conception can possibly be, so far as the constitution of my mind is concerned, and it is not the conception of being, but the conception of the absence of being—that is, of *nothing*. It must ever be very unsafe to reason from our shifting capabilities of conception. These are one thing to-day and another thing to-morrow.

We might make similar remarks on what is called the Absolute. That is properly the complete or perfect, knowing no defect or flaw. This perfection considered in itself is *nothing*. The word can only truthfully represent the mode of being in some object, and it must refer to certain properties of that object. For example, there is One absolutely good—that is, good without any mixture of badness. He is absolutely wise—that is, wise without any mixture of folly. And so on of every quality that goes to make up a *perfect Being*. If you speak of such an absolute as has no necessary relations, meaning such an absoluteness as must consist in literally every quality, good,

bad, and indifferent, then, though we say it with very great deference, we must think that you merely speak nonsense. We can no doubt think and speak nonsense, only the less we do so the better, especially when we seem to mean to speak philosophy. What is called "*the Unconditioned*," intending by the word to combine "the Infinite and the Absolute," deserves our attention on a similar principle. The "Conditioned" and the "Unconditioned," as mere abstractions, are *nothing*. This must not be lost sight of. It is in what is called the *concrete* that we see the positive absurdity of the notion. To be absolutely "*unconditioned*" is to be and yet not to be, for if one *is*, he is necessarily related to all else that is; and if he is not, he cannot be "*unconditioned*," nor anything else! In the sense of this term, as used by Sir William Hamilton and his followers, existence is just as impossible as it is that "yes" should be "no." For example, it must be existence without a mode of being; and yet it is asserted that its mode of being is this "*unconditioned*" one. Such a being cannot exist as a creator, for in this he must be relative to his creatures. But neither can he exist as necessarily not a creator, for this would imply his dependence on the absence of creative acts on his part! Is not this very notion of the "Unconditioned" as a mode of being, when taken in this absolute sense, as pure a *chimera* as ever was imagined? A black that is perfectly black and yet perfectly white is just as rational as a being thus absolutely "*unconditioned*." A nothing which is absolutely nothing, and yet is something, is just as real. Two and two that will always make five is a prince of an idea beside this "*unconditioned*" monstrosity. And yet it is under the spell of such follies that men are "*philosophically*" hindered from taking such views of God as are the groundwork of thought to the little child, who approaches Him with perfect confidence, that he shall not ask any good thing from his kind Heavenly Father in vain! It is needful, however, to come to closer quarters in this part of our controversy.

The three grand inconceivables of Mansel* are examples, and they are, I humbly think, only *blunders*. He says, "By the *First Cause* is meant that which produces all things and is itself produced by none." But a *first* cause which produces a cause cannot in the nature of the case produce "all things." That which has been itself produced as an efficient cause, produces the things of which it is the cause, as really as the unproduced cause produces those of which He is the Cause. The man who sins, and so produces things such as sinning produces, is as

* *Limits of Religious Thought*, page 90, edition 1858.

real a cause as He who produced him. Then again, if the First Cause had not produced causes, he would not have produced "all things." Mr. Mansel's definition is self-destructive. To speak of a First Cause as that which produces "*all things*," is either to speak most loosely, or to ignore the reality of created causes. Then Mr. Mansel says—"By *the Absolute* is meant that which exists in and by itself, having no necessary relations to any other being." So he says again, "a cause cannot, as such, be absolute," and when we ask why? he says—"the cause, as such, exists only in relation to its effect!" Is it the case that a cause, as such, cannot exist *without its effect*? Did not the Great First Cause exist before the creation he called into being? Is not that Being who, as a cause, uncaused himself, produces all other being, absolutely perfect as a cause; and is not his perfection, as such, demonstrated by such effects? Mr. Mansel and those who agree with him confound their own thinking, by introducing elements into their conceptions apparently for the sole purpose of making them inconceivable. What possible connection has the producing of *all things* with the conceptions of a *First Cause*? Does not a *First* presuppose a *second*? And what possible connection has the absence of all necessary relations, such as cause and effect, with the conceptions of the true *Absolute*? Is not the Absolute itself related to the non-absolute? The perfect surely stands in relation to the imperfect. Must we conceive of it as no longer perfect because it does so? Then as to the infinite, Mr. Mansel says—"By *The Infinite* is meant that which is free from all possible limitation—that than which a greater is inconceivable, and which consequently can receive no additional attribute or mode of existence which it had not from all eternity." Observe this "consequently." It is introduced as part of the definition of the Infinite. But the question is forced upon us—What connection has this *consequence* with the Infinite, so far as the additional "mode of existence" is concerned? Every thought of the Infinite mind is a mode of existence; but is it essential to infinity that no fresh thought should rise in that mind? Is divine unchangeableness a stereotyped eternal sameness in every mode of being? When philosophy runs itself up to this, has not philosophy run mad? "How can the Infinite become that which it was not at first?" Such is Mr. Mansel's question. And we ask what can hinder it? If this "Infinite" is not a mere absurd figment of the brain—if it is a living and thinking Being—if, as we know, it is God, Who only is the Infinite, why should not He become the Creator of the soul He forms to-day, and yet be still the Infinite, just because His power is equal

to all that may yet be His will, as it has been equal to all that is past?

Mr. Mansel is here in astonishing harmony with those who were, we must think, very different men. Thomas Hobbes and David Hume are remarkably at one with him in this matter.*

Mr. Mansel says:—"We are compelled, by the constitution of our minds, to believe in the existence of an Absolute and Infinite Being—a belief which seems forced upon us, as the complement of the relative and the finite. But the instant we attempt to analyse the ideas thus suggested to us, in the hope of attaining to an intelligible conception of them, we are on every side involved in inextricable confusion and contradiction."† This is not very promising, certainly. But is the case as Mr. Mansel represents it? We have not to go far with his strange argument till we see that the confusion is his simply, and not that of the truth regarding what he calls the Absolute and Infinite One. His first proof of the amazing statement which we have just quoted is that—"Distinction is necessarily limitation;" which we instantly deny. We distinguish an infinite object from a finite object, as we distinguish the abstract idea of infinity from that of limitation; but what ground is there for saying that by such a distinction we limit the one, any more than for saying that by the same distinction we render the other boundless? He says, "the Infinite cannot be distinguished as such from the Finite by the absence of any quality which the Finite possesses." That is, an infinite object cannot be distinguished as such from a finite object by the absence of limits in the one which are present in the other. Yet this is just how it is and must be distinguished. The Infinite object has no limits, which the Finite has. It is puerile to say that the infinite is a mere *negative*. It is negative only of the element of limitation. It affirms all the finite and infinitely more. It is Infinite only because of this negation of limit, and not because of the negation of anything else. Why may we not distinguish it as such by this very absence, which is its distinction, whether Mr. Mansel so distinguish it or not? Then he says that the Infinite "cannot be distinguished by any attribute which the Finite has not!" That is, an infinite object has no attributes which a finite object has not! Surely that whose mode of being is to be within bounds has not all the modes of being which that has

* See Hume's *Essays*, Vol. II., page 136, as to *Faith* and *Reason*. Also Hobbes's Works. Molesworth's edition of 1841, Vol. II., pp. 212, 216, &c.

† *Limits of Religious Thought*, page 45, edition 1859.

which has no bounds. These are attributes distinct enough, and the very attributes that mould our thoughts of each.

But it is, as we have seen, in Mr. Mansel's extravagant notions of what is meant philosophically by the "Infinite" in which we find the root of his confusion. He says — "The Infinite, if it is to be conceived at all, must be conceived as potentially everything and actually nothing!" He is clearly thinking of the Infinite in the abstract. But that is neither potentially nor actually anything. If what he says is true of infinity as a mode of existence, it must be true of the Being whose mode of existence it is. So God must be potentially everything and actually nothing! But what are the reasons given for this monstrous writing? "For," says Mr. Mansel, "if there is anything in general which it cannot become it is thereby limited; and if there is anything in particular which it actually is, it is thereby excluded from being any other thing." Again, we must remark that if he is writing of the abstract idea of infinity, it can become nothing in general, and it is nothing either in general or in particular. It can only be the manner of being to one who is infinite, and so in itself is nothing and can be nothing. If he is writing of the Infinite One, his language is unaccountable. Put in the concrete and applied to the only Infinite Being it says, that "if there is anything in general which *He* cannot become, He is thereby limited, and if there is anything in particular which he actually is, he is thereby excluded from being any other thing." He cannot become finite; is he thereby limited? To be finite is something in general which he cannot become, but in what amazing way can this set limits to His being? He is in this particular aspect or mode of His existence actually infinite, and cannot be anything else; but in what way does this limit Him? Is it possible to put greater absurdity in language than that we have quoted? But out of what does this absurdity spring? Out of the idea that to *think* of any object is to set limits to that object! So, to think of the Infinite is to set limits to Him, though in the very thought we put these limits away, and think of their *absence* as the grand distinction in the object thought of! Mr. Mansel says again, that "Whatever we conceive is, by the very act of conception, regarded as finite." So when we conceive of an object which has *no limits* we conceive of it as having limits!

When we ask ourselves what aim a writer can have in putting down such extraordinary sentences, it seems that Mr. Mansel imagines he is favouring true religion. But what is all this unaccountable logic intended to work out in favour of a truly religious state of mind? That all-

important point is stated in few words. Mr. Mansel says—“If all human attributes are conceived under the conditions of difference, and relation, and time, and personality, we cannot represent in thought any such attribute magnified to infinity; for this, again, is to conceive it as finite and infinite at the same time.” But where is there any difficulty in such a conception? It is not necessary to conceive of an object as infinite in *all* respects, because it is infinite in one. For example, it is not necessary to think God infinitely extended, because we think Him infinitely powerful. His omnipresence is not infinite extension: otherwise, the universe must be conceived of as infinite, as well as the Deity. But Mr. Mansel confounds all such distinctions, and leads on to the notion that “our soundest knowledge” of the Most High “is to know that we know Him not as indeed He is, neither can know Him: and our safest eloquence concerning Him is our silence, when we confess without confession that His glory is inexplicable, His greatness above our capacity and reach.”

If this meant no more than that our thoughts, as they are not infinite, cannot span the full greatness of God, it would be true, but it means that we really cannot judge of anything in God whatever! When, for example, it is said that “*God is love*,” we cannot, it seems, in the nature of things, know what the statement means! We cannot begin, as Christ teaches us, with the love of a prodigal’s father, and reason up to the heart of the absolute Father! We cannot know, it would appear, that what God feels is just what man feels, only God’s love is perfect and man’s every way imperfect! If a theological teacher shall demand that we believe in the most flat contradictions about God we are not to refuse, on rational grounds, because we cannot, on these grounds, know, whether his ideas are true or false! Is not this an attempt, by means of reason, to banish this very reason from the domain of theology? On the part of such writers as Hume it was the attempt to banish theology from the domain of reason. If the attempt is successful in either of its aspects, woe to the soul in which such success is secured. It is left destitute of all but an irrational faith.

There is a modification of these ideas which we have been discussing that constitutes a tremendous bar in the way of true prayer. It represents God, in virtue of His infinity and perfection, as so different from all that we think of Him when prayer seems reasonable, that belief in His responding to our requests must be groundless. The varied notions that go to constitute this bar generally combine in a certain idea

of the Divine *unchangeableness*. If He is regarded in His power over the material universe, that is imagined to have stamped on all matter such an impress, and to have so determined every line of movement from the first, that they can never be altered. If He is regarded in His omniscience, He is imagined to have so foreseen and ordered all, that there is no possibility of change at any point of the world's history. If He is regarded in His wisdom, it is assumed that it would be an impeachment of that wisdom, to think that everything has not been unchangeably fixed from all eternity. If He is regarded in His goodness, it is imagined to be utterly inconsistent with the eternal perfection of that goodness, to think that he will not do all which it is wise and right to do, without our asking him to do it. We are not in this case led into utter absurdity, such as we are brought to face in Mr. Mansel's contradictions; but into a region of metaphysical thought as to God, in which all is made to appear stereotyped and unalterable. True prayer with such a view is rationally impossible. We may go through a sort of exercise which we call prayer, and imagine that we are benefited in some way by that exercise; but the "ask and ye shall receive" of the Saviour's teaching disappears from our thoughts. Where lies the grand fallacy of this notion? It is found, as in all or almost all other cases, in this—*there has been an imperfect induction*. All the facts of the actual history have not been included. All *classes* of facts have not been taken into account. The Omnipotent has created at least one class of beings, one mode of whose existence is expressed by *will*. It is perfectly consistent with the highest idea of omnipotence to believe that He has done so. It would be inconsistent with such an idea to hold that He could not do so. In His omniscience He must have foreseen the perfectly free creature, whose mode of being would embrace this capability of will, and He must also have foreseen this freedom as truly as any of the acts that would flow from it. His wisdom can never be charged with anything so unwise, as the creation of a free creature without scope of really free action. But this would be the very unwise thing which He would have done, if He had created man, and *fixed* the succession of *every* event in the history of the very world in which He placed him. Such a contradiction would be as inconsistent with goodness as with the attribute of wisdom. The divine unchangeableness is not that of absolute sameness in the details of development, but that found in the principles on which that development takes place. Therefore we are shut up to believe, that the notion of everything being stereotyped,

or so unalterably arranged as to exclude all real answer to prayer, is a false notion. Finding the fact of man's freedom, we reason inevitably to that of God's suspending part of His acting upon the acting of the creature. This part of the Divine conduct is not fixed, and that because the perfect *principles* of the Divine character are fixed. God will do exactly that which is wise and good ; but what that shall actually be may depend on how the free creature will act in a given case. It is stated by Mr. Mansel as one of his proofs of contradictories in the Infinite that we cannot reconcile foreknowledge and free-will. I see no more difficulty in reconciling *fore*-knowledge and free-will than in reconciling *after*-knowledge and that free-will.

It is necessary to our freedom from such difficulty only that we have a somewhat clear idea of what foreknowledge really is, and especially of how it is affected by the futurity of that which is foreknown. Mistakes on these points no doubt cause great perplexity, but they are only mistakes, and may be easily corrected. Foreknowledge, like all other knowledge, is *thought*. It is such thought as is legitimately derived from the objects to which it is related. If, for example, I may say that I know the sun will appear above the horizon to-morrow morning at a certain hour, in doing so I merely express a thought legitimately derived from the evidence on which I anticipate the event referred to. It is a legitimate inference from certain facts of consciousness, that the sun will so appear, and hence I know that it will, just as I know or legitimately infer from certain other facts of consciousness, that it appeared to-day. If, to take a different case, I say of a man, who owes me a sum of money, and has engaged to pay me on a certain day, that I *know* he will do so at the time appointed, I merely express thoughts which are inferences from other facts of my consciousness, and are real knowledge *so far as they are legitimate inferences*. These thoughts are foreknowledge, as truly as thoughts of things past or present are ordinary knowledge of past or present.

But all such thought is affected essentially by the *futurity* of its objects. The thought of that which *is*, must be essentially different from the thought of that which as yet *is not*. The sunrise of to-morrow has no existence in fact to-day. My thought of that sunrise now, is that of a non-existent event. There is no corresponding reality in nature as yet, for the thought of that which is truly future. So the thought of the payment which has not yet been made, must be the thought of that which has as yet no reality. But this is not all. Events are con-

stantly occurring which need not occur, and which ought not to occur. To deny this is merely to beg the question of necessity as a universal law—it is to deny that either the creature or the Creator is really free. No act of a free agent need occur, and no act of sin ought to occur. The crime which is foreknown as one to be committed to-morrow need not be committed, and ought not to be so. It has as yet no existence—it may never be—and it ought never to be. That thought of it which we rightly call foreknowledge must embrace all this, or it is not knowledge, for it does not correspond with the event said to be known. There can therefore be no foreknowledge of that which depends for its occurrence on a really free agent, which does not imply the thought that it may never come to pass. This is not an affection of foreknowledge arising from the imperfection of the foreknowing mind. It is a necessary affection of all such knowledge arising from the nature of freedom and futurity. The more perfect the mind is which knows, the more certainly must these affect its knowledge. The mind of the Omniscient must, from its omniscience, think of the future as it is, and not as it cannot be. That mind cannot think of the future as if it were a past or a present, for the simple reason that it is neither the one nor the other. Nor can it think of that which may be, and yet may not be, as if it must be. Whatever the true nature of the future is, so of necessity must be the thought of it in the All Perfect mind of God. To say that that which He foreknows must come to pass, is merely to assert *necessity*, and so to deny freedom. If there is freedom, to the extent to which it is, to that extent there is no necessity, and God must know that there is none. He must know that the free act, which he foresees may be, may yet not be. He must know that the free act which he foretells may not occur. Some say it must occur, or his foreknowledge must be at fault and his predictions must fail; but this is only asserting that it is *necessary*, and that he foresees and foretells it *as necessary*. If he foresees and foretells it on the understanding that it is a matter of freedom, then, like Jonah's prediction of the destruction of Nineveh, it may not occur, though he has predicted that it should. There could be mistake in such a case only if the event were foreseen and foretold as *necessary*.

The true difficulty to which Mr. Mansel refers is simply that of reconciling necessity with freedom, so that an event must be, and yet need not be. No doubt that difficulty is great enough, but it need not hamper philosophy any more than the difficulty of regarding something and nothing as the same. Freedom is foreknown as freedom, and necessity as

necessity ; and, if we only keep our ideas of both distinct, we need feel no difficulty in reconciling both with foreknowledge, even as that is in the Divine mind.*

The great practical question will be found in the end to be this—what has the Great Dispenser determined as to the conditions on which He will act ? Has He made His action in any degree dependent upon man's asking ?

But this belongs to moral rather than to metaphysical science. It is clear that there is no physical or metaphysical difficulty in the way of such a suspension. The difficulties appearing to exist are purely imaginary, and the fruit of modes of reasoning whose defects are transparent the moment we take *all* the facts of the case into consideration. Here, as in many other matters, we find a defective science, or a defective logic rather, at the foundation of objections that look terribly formidable in their bearing against Christian truth. The flagrant fault is in the "science." Fault there is none in the Bible doctrine.

At this point we come upon the question as to *miracles*. Is a miracle a suspension of natural law ? Hume says, "A miracle is a violation of the laws of nature."† It suits his purpose to say so. However clear our view is of God's agency as actual, and as to a certain extent depending in its acting on human action, we are strongly constrained to believe in His adherence to law. Consequently, when a careful thinker is told of a suspension or infraction of natural law on the part of the Divine Agent, he cannot help feeling as if a serious difficulty were thrown in his way. It is this which we think gives Hume's celebrated argument against miracles the power it has wielded over credulous minds. He says that "a firm and unalterable experience has established these laws." The *fulcrum* on which he rests his lever is what he thus calls "experience." And it cannot be denied that, so far as history records the experience of man, it is no easy matter to find in it a recorded instance of suspension or infraction of a true natural law. If that history records anything it records miracles, but those miracles which it does record are neither suspensions nor infractions of either natural or moral law. Hume is not entirely free from all suspicion of dishonesty, however, in this. He confounds

* The best view of "Divine Prescience" I have seen, is given by Mr. Reddie in his *Fresh Springs of Truth*,—London : C. Griffin & Co. 1865 (pp. 168-179),—a little volume of exceedingly courageous, yet cautious and valuable thought.—J. K.

† *Hume's Essays*, Vol. II., pp. 120, 133, 138, Ed. 1800.

usage with law ; or, rather, he reasons as if the usual course of nature observed by us were equivalent to natural law. The progress of being which we have already noticed is fatal to his mistake. If his argument, consequently, has any force, that force lies in our experience of law, and not of temporary usage. There is no violation of any *law* of nature in any of the miracles of the Bible, though there is in some of them a departure from usage.

Take the case of Christ walking on the Sea of Galilee, and enabling Peter to do the same. Is there in this any suspension or infraction of natural law ? Does any one say that gravitation was suspended ? Then what kept the two bodies from flying off from the surface on which they walked ! If I wade through a stream, and, as I do so, I bear any object that I have with me above the surface of the water, do I suspend or violate the law of gravitation ? Clearly no. I only exert another force sufficient at the time to keep the object I am carrying above the surface. Take, again, the case of the "*withered arm*." When by an unusual exertion of power the Saviour made the living action pass through that arm, did he suspend or violate any natural law ? We can see no such suspension or violation. We can see an exertion of force which is unusual, but that force is exerted in perfect accordance with all the laws which it ever follows in its most ordinary exertions. The "*vis vitæ*" of the materialist passes from the ganglions, along the various tissues, and affects arteries, veins, muscles, bones, skin, and all else, in perfect accordance with law. Take the dead body that had "lain four days" in the tomb, and let the same thing be done to that which is done in this withered arm, and where is either the suspension or infraction of any one law of nature ? Hume's gathering up of his argument is in these words :— "It is experience only which gives authority to human testimony, and it is the same experience which assures us of the laws of nature. When, therefore, these two kinds of experience are contrary, we have nothing to do but subtract the one from the other and embrace an opinion either on one side or the other, with that assurance which arises from the remainder." Who does not see that this vaunted argument goes to smoke, the instant we perceive that no real miracle involves the slightest deviation from natural law ? If it shall be said that usage is violated, we have only to ask if it is contrary to human experience that it should be so ? Is not every variation in nature a departure from usage ? What was that leap which Sir Charles Lyell contemplates when he says, "We may also demur to the assumption that the hypothesis of variation and natural selection obliges us to assume that there was an

absolutely insensible passage from the highest intelligence of the inferior animals to the improvable reason of man.”* The departure from usage in which a human being should be born of one of the lower animals would surely be departure enough from what Hume calls experience! And yet that is only an idea produced (in one who has had a very wide experience), by the departures from usage that are in nature. These, however, are no violation of law. *Neither are the greatest of Scripture miracles.* Take a case to our purpose in this inquiry as to prayer. “Elijah was a man of like passions with ourselves, and he prayed that it might not rain.” What natural law did he wish suspended? Is the absence of rain the suspension of some natural law? Can Hume’s experience, or that of any one else, point out the law of which it is either the suspension or the infraction? But Elijah prayed again that it might rain. And when that cloud, no bigger than a man’s hand, at length rose on the horizon, was some natural law broken or suspended? There is not a shadow of a ground for saying so. Human experience of natural law was as perfect all through that famine, and at the close of it when the rain came, as it ever had been; but the *miracle* was not the less real on that account. That agent, by whose power the heavens give rain and withhold it, acted in this case, as in all cases, in perfect accordance with everything that can be called *law*, whether in the sphere of matter or in that of mind: so Hume’s great argument is only a great blunder. Hume was fortified in his error by his ideas of “antecedence and consequence” as all that we know of cause and effect; but even here his foundation was a blunder as to fact. He took it for granted that man’s “experience” of “antecedence and consequence” in nature has been that of uniformity, which, as we have already shown, is *palpably and egregiously untrue*. When we are asked, therefore, if we expect God to work a miracle in response to our requests, we may reply by asking—what if he should? If it is asked again, if we think He will violate His natural laws to answer us, we may reply that there is no need for any such violation. We can think of nothing we could for a moment desire that would call for his departure by the slightest conceivable degree from any one of these laws.

If we epitomize our discussion and follow out the sound principle on which all the facts of the case come under review, we find ourselves surrounded by a very clear atmosphere of thought as to our great subject. Minds everywhere we see

* *Antiquity of Man*, p. 504, Ed. 1863.

have power to change material things. Minds have power also, to a certain extent, to change other minds, and so to change these other minds as to lead to the change of material things by their mediate agency. If we take any great work which has been effected by men, and go back into its real history so as to note the facts of that history, all this at least is irresistibly manifest. Say it is a great viaduct that now spans a valley, and we run rapidly back over all the occurrences that have issued as their combined result in this vast work, till we reach the first thought to which it can be traced in an individual mind : we have in those facts, beyond all question, instances in which minds acted upon material things—instances in which minds acted on other minds so that these again acted on material things—and instances, moreover, in which chains of minds acted on each other and led to material, as the result of mental, changes. Among these facts we find *askings* as really as any other facts whatever—we find *givings* following those askings—we find *receivings* following those givings ; we find no fact of any kind in the universe that is more real than those *askings*, *givings*, and *receivings*. There is no antecedence or consequence more evident, than that which holds good between those said askings, givings, and receivings. Not that the antecedence and consequence are uniform, for there are *refusings* following askings as well as *givings* ; but with all the lack of uniformity, no one can doubt that in myriads of cases the giving follows the asking as its effect, and is as evidently that effect as is any other consequent the effect of any other antecedent whatever. But among the facts with which we find ourselves surrounded are *askings directed to God*. What is the sole element of difference in the case of these askings ? Matter is matter in this case as in every other in which it is involved—mind is mind also in this case as in every other—only in this case one mind is perfect ; in all others the minds asking and those supplicated are imperfect. Call this perfection infinite, absolute, anything you choose—your words make no alteration on that mind which has all possible qualities that go to make up a Perfect Being. And now comes the question—*Is one of these qualities that of insensibility to askings ?* Beyond the possibility of dispute the askings are there—the *sensibility to the askings and the givings alone are denied*. Man acts upon matter, and upon mind too, when requested to do so. Man refuses to act on matter, and also on mind, though requested to do so. *Is it essential to his coming nearer perfection that he should always refuse ?* No one will say so. Is it essential, then, to the perfection of God that He should *always refuse* ? Is deafness to entreaty a perfection ?

Is the statue of a mother, to which the infant cries in vain, a more perfect being than the *living mother* who acts on the instant the wail reaches her ear? Would it be an element of perfection in God, to be like the statue and unlike the living mother? If true philosophy could annihilate the *facts* of asking, it might greatly alter the case. But it refuses to ignore or alter a single *fact*. Even a falsehood is a fact to a real philosophy; though its object is unreal, it is real itself, and should be weighed as carefully as any other fact. Consequently philosophy is intensely interested in these *askings which we call prayers*—THEY ARE FACTS. They point us irresistibly upward to the All-Perfect One, and compel us to believe either in His giving or in His refusing. He either acts as requested or He does not act. True science leads us to look to other fields of inquiry, and to ask what the facts which lie in them teach us as to His responding, or refusing to respond, to the movements of his creatures. If we till and sow, our labour is worthless, unless One who has command of sun and rain respond. Does He respond? Not so uniformly as to sanction the mechanical idea of His great universe—yet He does respond sufficiently to give perfect confidence to the good husbandman and to call forth the gratitude of every intelligent heart. If we *ask*, does He respond? Not so as to sanction the idea that asking is everything that is required in order to our receiving; but yet he has so responded, as to have kept asking alive in human beings through all the centuries of their stay on earth. Here, however, our work for the present closes. We have traced the outline of the relations to which we have directed attention in Metaphysical and Physical Science, leading along the path of those relations into that field of thought in which we find the needy suppliant asking of the Heavenly Father, and receiving from Him "*that which is good.*" We have found that true science is in perfect accord with such asking, such giving, and such receiving, as are involved in the Christian Doctrine of Prayer. Instead of requiring to lay aside "*reason*" in behalf of "*faith*," we find the severest logic leading us on to that fellowship with God, which, as man is constituted, is impossible without that interchange of heart between the Divine Helper and the needy children of men, which takes place in sincere supplication on the one side and merciful and gracious giving on the other.

The PRESIDENT.—Ladies and Gentlemen, it is my duty to move a vote of thanks to the author of this paper, and to express to him our deep gratitude for the diligence, care, and profound thought exhibited in it. It would be presumption for me to say I could follow the paper throughout; but in the

after part of it I entered into the arguments without difficulty, and, considering the great value, in the days we live, of having such subjects thoroughly gone into, I am sure that all here, without any critical examination of the differences of opinion that may well exist upon some points, will join heartily in saying that the learned author of the paper is entitled to our utmost respect and gratitude. (Hear, hear.)

Mr. WARINGTON.—My Lord, we have little time left, and I will therefore begin what I have to say at once, so as to leave as much time for others as possible. It is unpleasant, after the beautiful finish of Professor Kirk's paper, with which all of us must so heartily agree, to turn back to the drier matter of its commencement in the way of criticism ; and yet I am sure Professor Kirk would wish his paper to be criticised, and therefore I do not hesitate to set about the task. Professor Kirk begins with a long metaphysical introduction ; it seemed to me somewhat unnecessary, as being a kind of introduction equally appropriate to every subject whatever which we have to discuss. We must know what "knowing" is before discussing any part of knowledge, and I do not see how it is more needed here than in subjects generally. Passing now to details, I cannot but think there were one or two cases in which our author was rather hair-splitting in his criticism of other writers, and especially of Mr. John Stuart Mill. I am no advocate for Mr. J. S. Mill, and should dissent from his philosophy as much as Professor Kirk does ; but I think Professor Kirk has dealt with him somewhat unfairly, and strained several of his expressions in a manner very undeserved. But of this more presently. I notice, also, one or two scientific errors in the Paper. For instance, Professor Kirk speaks of light as a movement in the atmosphere. Now, light passes with equal ease through a vacuum, and is therefore plainly *not* a movement in the atmosphere. It passes also with ease through transparent solids or liquids in which there is no air. Yet so completely is this erroneous idea ingrained in the Professor's mind, that he speaks of "ether" as being now regarded by philosophers as a nonentity. I should like to know the modern philosopher who thinks so——

Rev. W. MITCHELL.—Does not Professor Grove do so ? I rather think in his last work he does.

Mr. WARINGTON.—It may be so ; but I was not aware of it. Then as to the criticism which Professor Kirk gives as to what we mean by "I know." He seems to take it for granted that it must be an action of the mind on something. I confess I do not see why the expression may not have the same sense as "I see, I feel, I hear, I smell," in every one of which cases there is reference to an impression made on ourselves by something without. It is surely false logic to say that because we have certain words, as "pain, warm," &c., which are construed in a more active sense, therefore we may not take "I know" as to be construed in the same manner as "I see, I feel," &c. I do not see any reason why it should not come under this category rather than the other. Professor Kirk chooses to define the verb "to know" in a different sense from that adopted by J. S. Mill and others, which difference in definition constitutes the whole of his criticism, without any reason to

support it, confidently as he may affirm the other sense to be utterly false, and not to include anything resembling knowledge at all. The fact is, there are two kinds of knowledge—knowledge of perception and knowledge of reflection. Mr. Mill takes perception as the essential part of knowledge; Professor Kirk, on the contrary, regards knowledge as exclusively reflection. He is, of course, at liberty to take the word in any sense he likes; but to abuse another for taking it in a different sense seems to me rather unfair. Then we come to the question of what our knowledge really consists of. Do we know anything beside the impressions received through our senses? Professor Kirk says we do—we know something over and above our perceptions or sensations. I am at a loss to know through what medium this further knowledge comes. It is not by seeing or by hearing, by smelling, by tasting, or by feeling—how then? In what other way but these is it possible for us to come into contact with external objects? Is there a sixth sense? If so, what is it? If there is not a sixth sense, but only five, and all our knowledge of external matter must come through one or other of those five, then the assertion is perfectly correct that we know nothing of external matter but from the impressions conveyed to us through our senses. It does not follow from this that we are therefore to dwell on these impressions as if *they* were the proper subjects of knowledge; not by any means. We believe, and are right in believing, that these impressions are truthful, *i.e.*, that there is a reality existing which is the cause of the impressions. (Hear, hear.) We fix our minds on that reality then as the true subject of knowledge; but still it remains true that we know nothing of that reality but *through* the impressions. The relation of man to external nature is, in fact, much the same as that of a general to an army, concerning which he receives intelligence only through his *aides-de-camp*. He receives reports of the different movements going on, the positions of the enemy, and so forth; and knows and can know nothing of what is going on but through these reports. Yet when he receives one of these reports he does not reason on it, and deal with it *as* a report, but rather fixes his whole attention on the facts reported, and shuts the report as such out of his head altogether; if, that is, he believes it to be true. (Hear, hear.) Just in the same way we fix our attention on the objects perceived, not on the perceptions by which we obtain our knowledge, while yet we all the time know nothing of the objects but that which comes to us through our perceptions. The question is not, as Professor Kirk puts it, of a mere sequence between sensation and knowledge, or sensation and inference; but it is a question of possibility of thought. What possibility have we of obtaining knowledge of anything without us but through our senses? If there is no such possibility, then Mr. Mill is quite correct in saying that the impressions received by the senses constitute the whole amount of our knowledge, or, to speak more accurately, the materials for our knowledge (hear, hear); and as in one of his statements he speaks of knowledge as consisting of our conscious sensations and the legitimate *inferences* from them, I apprehend that the difference which appears to lie on the surface is unintentional, and Mr. Mill's opinion the same as that which all

reasonable persons hold. Then I notice Professor Kirk refers, in a part of the paper which Mr. Reddie passed over, to modes of existence. He says :—

“It is necessary to be careful that we really understand what we mean by a *mode* of existence. We get at this by passing from the mere abstract idea of a *mode*, or *manner*, to the concrete idea of the mode or manner of being in a particular object.”

I hope that is a misprint, because the process is in reality just the reverse ; we first get the idea of a mode in a concrete object, and then make our abstract. Then as to that illustration, which seems so taking, concerning the gunpowder, and the inference that because power in one case is exhibited on the insertion of a red-hot wire, which is not exhibited in the other, therefore there is some substance present in the one which is absent in the other. Let us alter the circumstances slightly. Suppose we take, in one case, powder in an early stage of its manufacture, when in the form of a solid cake, and we insert a red-hot wire, it also does not explode. We take, however, the same powder, of exactly the same composition, made at the same place, and by the same people, a piece, if you will, of the same cake ; we grind it into small particles, we insert the red-hot wire, it explodes. Now, if Professor Kirk's argument is logical, we are bound to conclude that there is a distinct substance present in the one case which is not present in the other. The argument leads to a false conclusion ; it cannot, then, be true. (Hear, hear.)

MR. REDDIE.—There is another substance present. There is air between the granulations, after the cake is powdered.

PROFESSOR OLIVER BYRNE.—And it does not become powder until it is milled.

MR. WARINGTON.—It is the same substance exactly——

MR. REDDIE.—It is not *powder* !

MR. WARINGTON.—I merely take this illustration because it is the one which Professor Kirk himself selects. Let me add another. I take a piece of iron which has been magnetized, and another which has not been magnetized. Now, you will remember Professor Kirk lays down as a principle of science, that magnetism and its cognate forces are not entities, but mere modes of existence. In the case of these two pieces of iron, then, the only difference between them is in their mode of existence. There is no substance, according to Professor Kirk, present in the one which is not present in the other, since he denies that there is any substantial entity in magnetism——

PROFESSOR BYRNE.—You cannot trace the magnetism without the iron.

MR. WARINGTON.—Now, in this case, if any one compared the two pieces, he finds at once a property present in the one which is absent from the other. If he applies a bit of iron to the one, it is held fast ; if to the other, it is not. Would not the legitimate inference, then, be, if this line of argument is sound, that there was some substance present in the one which was absent from the other ? Yet, according to Professor Kirk's principle, this would be false, since magnetism is no substance whatever. I am not saying

that the particular conclusion drawn in the paper is false, nor the line of argument adopted essentially illogical, but simply that in the form in which he puts it, it is a false one, since it leads to a false conclusion. Then, as to the criticism on Professor Grove, as to motion and force. Professor Kirk says that what is called force is admitted to be nothing more than motion. Now, Professor Grove and other scientific men hold that as firmly as Professor Kirk himself. Why, then, do they call it now motion and now force? Because it is regarded in two different aspects. Regarded as existing in any particular thing it is motion. Regarded as passing on into something else, and thereby producing a change in that something, it is force, simply because you look at it under another aspect. If I take a hot bar of iron, and regard it in itself, I say, This iron is in a state of motion. If, now, I bring my hand near to it, I receive part of that motion; it confers motion upon me, it causes the particles of my hand to move also, and so exercises force, and this I apprehend is all that Professor Grove or any one else intends by force as distinguished from motion. Now we come to the great point of the paper, that mind is the true generator of force. Is this so? Let us take the illustration Professor Kirk dwells upon, this delicately-arranged experiment of Professor Grove, in which the raising of a shutter by the hand causes certain changes to take place. Is that raising of the shutter the cause of those changes? Alter the circumstances very slightly, and you will see in an instant that it is not. If a thing is really the cause of any phenomenon, the omission of that thing will inevitably occasion the non-occurrence of the phenomenon. If, then, here, the same effect can be produced without any human being lifting the shutter, it is plain that lifting the shutter is not the efficient cause. Let us suppose the apparatus arranged without a shutter at all, in a dark room, and left to itself. A flash of lightning comes, it is sufficient, all the phenomena are produced, and yet no human being has had anything to do with it. It is plain, then, that the lifting of the shutter in this experiment is not the efficient cause of the phenomena which result, because these phenomena can result as well without the shutter being lifted at all—

MR. REDDIE.—In that case you must attribute it to another *mind* that caused the lightning. (Hear, hear.)

MR. WARINGTON.—I repeat, then, the lifting of the shutter is not the cause. What is the cause? The cause is the *light*. It is the light which produces every effect which is seen, and the work which *mind* has to do is simply this—to control at what particular moment, or under what circumstances, the light shall come. The mind does not occasion the light; it simply controls when and how it shall come, directs its path, and so causes it to effect certain objects. The real acting influence is the light, the mind is only directive. But now, to take the other aspect of the illustration. Man, at all events, had arranged the apparatus in order to produce the effect. True; but by what power had he arranged it? By the power of his muscles. And whence came that power? Solely from the combustion of a certain part of his own frame, which he had no power to occasion or to stay.

It is going on always, and all he can do is to *direct* this muscular force, so as by it to attain certain ends ; and except he thus directs it, of course those ends are not attained. (Hear, hear.) It is important that we should know exactly how it is that mind is essential, whether as the directing or the efficient cause. Professor Kirk seems to think as the efficient cause ; but it seems to me only as the directing cause. In the same way, for instance, if I want to light the gas. You may say it is my putting the match to the burner and turning on the gas which causes the flame ; but, no, I may do that as often as I please and effect nothing, if there is no gas in the pipe. The cause of the flame is the combustion of the gas. I simply direct and control when and how it shall take place ; but I am absolutely powerless to cause it except I have all the forces and materials at my command by which the effect is produced. Next, as to the question of motives—how far the motives which control the human will are themselves occasioned by the circumstances in which the man who wills is placed. Professor Kirk argues that they are not so occasioned, because it does not necessarily follow on any given circumstance that the same result shall follow. Now, of course, this theory cannot be expected to hold good in such a case except *every one* of the circumstances present on the first occasion are also present on the second ; and how seldom, if ever, can this be ! Again, it is to be remembered that a man's action is the result, not of one motive acting alone, but of a whole series of motives variously counterbalancing each other. We find the same thing takes place in the natural world. We know that many forces are acting at the same time on every object, and what occurs to that object is the result of all the forces together, and not of any one in particular. Professor Kirk says :—

“The same assemblage of conditions which are in one instance followed by one volition are in other cases followed by its opposite.”

I doubt whether he could bring us a case of the same assemblage of conditions. I should think it was almost impossible to take two men, or even the same man, on two occasions, and expose them to exactly the same influences and conditions, so as to see if the result would be the same. Next, I notice that, further on in the paper, Professor Kirk alters his tone as to the will being an efficient force, and grants that the will is limited, and requires certain powers at its disposal to effect its purposes. He says :—

“To be a Creator of worlds implies powers by which the will may be carried out into this result.”

And again, on the next page :—

“It is no drawback to this argument to say that matter resists and often overwhelms man, because that proves only that man's power to move and change matter is limited.”

Then, a few words as to the variableness which he insists upon in the organic world, and which, he holds, puts the organic world on a different

footing to the inorganic. How is this occasioned? Simply, I should say, by the extraordinary complexity of the conditions which determine the course of events. A number of seeds from the same pod are put in different places, and grow differently. Very true; yet I should be disposed to regard the growth of these plants as being as absolutely regulated by law as any chemical combination, only the law is more complex, the result dependent on a far larger number of minute circumstances, so that it does not appear so uniform, though it may really be completely under the control of law all the time. He says:—

“If we are told that the changes in the natural world take place according to an invariable order of succession, and that this is the fixed law of nature, we are told what is *transparently untrue*. If such a statement is made in the name of scientific culture, it is made by one who is himself ignorant of one of the most irresistible conclusions of science.”

What is the reason that men of science make such an assumption? Simply because, in cases which appear at first sight to have this kind of variability, the progress of science has shown that they are really subject to law; and so analogy would lead us to suspect the same thing in other quarters. For example, of old it was considered that nothing was more variable than the winds; in the New Testament the wind was taken as a type of that which came and went where it listed, yet there is no doubt that the course of science is tending to exhibit these very winds as a result of uniform laws and causes, only the conditions under which these causes act are so complex that they do not appear on the surface to produce a uniform result. In the same way we may expect that the apparent variableness in the vegetable and animal worlds will be found to be as subject to law as the more manifest uniformity of the inorganic world. Simply stating my opinion, I should be inclined to say that the only exception to uniformity is man himself, and that because man is not in harmony with nature, and does not carry on his part in the universe in the manner intended; he is not acted upon by circumstances as he was meant to be, but follows his own will, and is thus the only exception to the great reign of law. I should be disposed, therefore, in spite of Professor Kirk, to hold that what he tells me is untrue, and to declare myself “ignorant of one of the most irresistible conclusions of science;” and I take his epithets cheerfully because I know that they are in this matter quite undeserved. Then, as regards his criticism upon Professor Mansel, as to the knowledge of the infinite and absolute. With the greater part I agree; but I notice one sentence towards the close, which I cannot pass over:—

“Mr. Mansel says again that ‘whatever we conceive is, by the very act of conception, regarded as finite.’ So when we conceive of an object which has *no limits*, we conceive of it as having limits!”

But *can* we conceive of an object having no limits? I have tried hard, and my experience is, that we cannot; and the reason is, that every notion we form in our minds must first come to us as a perception through our senses.

We know no quality as existing which we have not perceived in some concrete being ; for example, we should never know a colour if we had not seen it, and we can form no idea of it until we *have* seen it with our senses. All our notions, therefore, of existing things are limited by our *perceptions* of their qualities. Have we, then, ever come in contact with any existence in such a way that we can perceive its infinity ? We have thus come in contact with finity ; but I certainly never have with infinity, and I doubt much whether any one else has——

Rev. W. MITCHELL.—I think we did the other night. I gave a small demonstration.*

Mr. WARINGTON.—I wish I had been here ; it would have been quite a new sensation ! But it will be said infinity is not a positive, but a negative quality. What, then, is its inevitable characteristic ? That it is limited as a quality by that of which it is a negative. For example, if I name the quality non-redness, I am simply negating redness as far as I know redness ; and I can do no more, for I cannot negative that which I do not know ; my negation is strictly limited by its corresponding positive. So, when I negative finiteness, all I can say is, I have stretched my reason to the very utmost point as regards extension, and still my conception is bounded, still I have got limits ; I believe that my conception herein is untrue, I believe there are *no* limits. Have I grasped the infinite ? No. I have simply denied that anything I can conceive is a sufficient measure of that which really exists ; but as to getting the measure of that, there you utterly fail. At the same time, the application which Professor Mansel makes of that argument is, it seems to me, utterly erroneous ; for he says, because we cannot get a full measure, a perfect conception, therefore we cannot get a true idea at all. But I do not see why, if I have not full knowledge of extension, my knowledge, so far as it goes, is therefore not true. Or why, if I have an imperfect knowledge of love, and cannot grasp its full measure, my knowledge of love should not be a true one so far as it goes. And if so, why must I not have a true knowledge of God, although I grasp not the infinite, the absolute, the First Cause ? (Hear, hear.) But now, to come to the real essence of the paper, the difficulty of reconciling together the uniformity of nature with the effectiveness of prayer. Taking up that thought which I threw out just now, that man alone is out of harmony with nature, what is necessary in order that man should receive those blessings which God originally designed for him ? Why, simply this, that he should place himself *in harmony* with nature and God. And is not that exactly the true efficiency of prayer ? Man by prayer places himself once more in his true position towards God, in such a position, therefore, that he can receive what

* Mr. Mitchell referred to a model by means of which he showed at the last ordinary Meeting the passage of one crystalline form bounded by 8 faces through an *infinite* variety of other forms bounded by 24 faces, and then to another bounded by 12 faces only ; thus visibly producing an *infinite* series of forms in one second of time, and within a finite space.

God originally designed to give him, and this without any infraction of law, but by restoring the true harmony of relation. I am not putting this forth as a new idea, for I believe it is in essence the same as that advocated by Professor Kirk, only expressed in other words. But what is it that is meant by law? I do not think we can take law to exclude usage, as Professor Kirk wishes; that is, the generalization of an observed order of phenomena. We observe a certain thing always follows on something else. And such a generalization we call a law. Although we do not know why it follows, yet we call it a law. But this surely includes *usage* as much as anything else. It seems to me, therefore, that we must still hold that miracles are an infraction and suspension of natural law, in the ordinary sense of the word. It is contrary to the law of nature, as far as we have means of knowing it, that a man by his voice should call another from his grave. The means by which it is done may be in accordance with law. I believe that, in cases of this kind, we should expect to find God violating law to as little extent as possible; but still there is a violation of law: it is not a natural thing that a man's voice should be sufficient. If you say it is no infraction, I fail to see how, in such an event, you get any proof of the supernatural. If you say you do not know what is natural, I fail to see how we are ever to know which are miracles and which not. At the same time, we must bear in mind that law with us is not an absolute thing, but relative. There may be far higher laws, of which we know nothing, and we have therefore no right to say that God is infringing law absolutely, but simply natural law as known to us. (Hear, hear.) One hint, in conclusion, as to the way in which the comparison is drawn between man's prayer to his fellow-man and man's prayer to his Maker. I think the analogy between the two has been put too strongly by Professor Kirk. And in this way everything which a man asks his fellow-man is not within his power, even if within his wish, because he has only a limited authority over nature. He has to conform himself to the laws of nature. Now, what are these laws? I believe that these laws of nature are simply our mode of expressing the uniformity which marks God's constant and immediate action upon nature. I do not think we have any right to suppose they are laws implanted and imposed by God on matter, but rather the natural tokens of His own immediate working. Now, grant that to be the true meaning of law, you see at once how different are the two cases. Man, in order to grant any request, must bring himself into conformity with those laws produced by God's immediate action. God has no limits, there is no difficulty on His part, no possibility of infraction of law, because the law is simply Himself, and He cannot infringe His own nature. The difficulty of the question that appears to arise from the existence of natural laws and the uniformity of nature thus falls away entirely, and we perceive that the answer to prayer is really the proper and inevitable result of that same unchangeableness of the Divine nature to which the uniformity is due.

Rev. W. MITCHELL.—At this late hour of the evening I feel it necessary to make my observations as brief as possible. All must acknowledge that Professor Kirk has given us a most important paper on a most important

subject. I well remember the astonishment with which I read, about a year ago, Professor Tyndall's objections to prayer, in his popular work on the Glaciers of Switzerland. He asks, with something like a tone of contempt, how a priest could be so ignorant as to pray for a change of weather. That if he only knew the laws of natural philosophy, he might just as well pray for a miracle that should cause water to run up-hill, as to pray for rain in a time of drought. That the fall of rain was a matter dependent on the position of the gulf stream, the direction of the trade winds, and other things governed by laws as inexorable as those which prevented water running up-hill;* and, therefore, to pray for fine weather or for rain in a time of necessity was what no highly-cultivated philosopher could do. If this be so, the prayer taught us at our mother's knee from childhood—the petition we address to an All-powerful Father, "Give us this day our daily bread," is one no natural philosopher can ask, nor any one with a highly-cultivated and philosophical mind. Now, if I wanted an antidote for this scepticism, for a man unlearned in natural or metaphysical philosophy, I would refer him to the study of Professor Kirk's paper, which we have heard read this evening. I say this, not as agreeing with every argument used in that paper, but on account of its main scope. Mr. Warington has criticised with great fairness and clearness many portions of the paper. But such differences of opinion only point out the difficulties of the subject of discussion—difficulties which enter more or less into every subject involving metaphysical considerations. I am prepared to maintain, in opposition to Professor Tyndall, that the cause of any scientific man's scepticism as to the power of prayer arises not from strictly physical, but from metaphysical, difficulties. These difficulties are metaphysical subtleties, the cobwebs men have woven out of their own imperfect minds and imaginations, and set up as incontestable verities. I think Professor Kirk's paper shows that all the philosophical objections urged against prayer resolve themselves into purely metaphysical considerations. You cannot discuss the questions touched upon by Professor Kirk without finding that the scientific objections urged against prayer are not difficulties arising from any truth revealed by God's works, but mere metaphysical puzzles

* Professor Tyndall has repeated his philosophical objections to such prayers as are here alluded to, in the following passage, which concludes his paper on "The Constitution of the Universe" in the *Fortnightly Review*. "A miracle is strictly defined as an invasion of the law of the conservation of energy. To create or annihilate matter would be deemed on all hands a miracle; the creation or annihilation of energy would be equally a miracle to those who understand the principle of conservation. Hence arises the scepticism of scientific men when called upon to join in national prayer for changes in the economy of nature. Those who devise such prayers admit that the age of miracles is past, and in the same breath they petition for the performance of miracles. They ask for fair weather and for rain, but they do not ask that water may flow up-hill; while the man of science clearly sees that the granting of the one petition would be just as much an infringement of the law of conservation as the granting of the other. Holding this last to be permanent, he prays for neither."—W. M.

woven by the imperfect brains of men. Were I to venture on a criticism of Professor Kirk's paper, it would be as to his division of all substances (using that term in its metaphysical sense) into mind and matter. In this division, though with great diffidence, I should be disposed to differ from him. I believe there are other verities or existences in God's universe besides mind and matter. I believe forces of various kinds have a real existence ; that many of these can neither be resolved into mind (meaning by that term an intelligent substance) nor matter. All that we know of force and matter, so far as mathematical demonstration is concerned, lies in a very narrow compass. For all the purposes of mathematics and of demonstrable natural philosophy, a very simple definition of force and matter suffices. Whatever moves or can be moved is matter, and whatever can cause matter to move is force ; but when we quit the domains of pure mathematical demonstration, we soon become involved in purely metaphysical difficulties, those difficulties which are leading, as I believe, such philosophers as Professor Tyndall and Mr. Grove astray. The tendency of natural philosophers who quit the region of pure mathematical demonstration is to confound force and matter as things which are identical instead of being distinct from each other. This has ever been the course of the metaphysical rather than the physical reasoner. The purely physical reasoner has a distinct conception of force and matter as two very different existences which cannot be confounded together. The metaphysical reasoner who would pass beyond the rough practical distinction of force and matter which satisfies all the problems of the physicist is involved at once in metaphysical difficulties. The essence of matter evades all his researches ; he meets everywhere the evidence of force ; and the effect of force alone is all that his senses convey to his intelligence. He, therefore, as Boscovich did, resolves all matter into what he calls centres of force, and so, quite as effectually as Berkeley, the pure metaphysician banishes all matter from nature. Hence, therefore, metaphysical researches would effectually banish all matter from existence, and land us in a universe of pure force, or what I presume Professor Kirk would denominate pure mind. But do not such metaphysical considerations as these banish, not only the inductions of common sense, but all the real knowledge we have acquired ? I cannot prove the existence of matter metaphysically any more than I can prove the existence of mind. There is, however, a practical way of resolving these metaphysical subtleties. If I doubt the existence of matter, I have only to run my head against a wall to get a demonstration that will at once rudely banish any scepticism induced by metaphysical arguments. We have, I believe, as good evidence for the existence of matter as we have for the existence of force ; and as good evidence that force and matter are distinct entities as we have for the existence of either. When we enquire, however, whether force is inseparable from matter ; whether all matter is not endowed with force and whether there are not forces completely separated from, and not co-existent with, matter, we come upon most debateable subjects far removed from the bounds of strict logical demonstration. If by mind we are to understand an intelligent

substance—using the word substance in its metaphysical sense—I cannot agree with Professor Kirk in his assertion that mind alone is the cause of motion. I believe that intermediate, as it were, between mind and matter, there are forces which are not intelligent agents. Though in this I do agree with him: that all motions or changes induced in the material world by the action of laws regulating the motion and combinations of particles of matter, are ultimately resolvable into the will of that mind—(hear, hear)—namely, that Spiritual Being whom we acknowledge not only as the Great First Cause, but also the Supporter and Sustainer of all things. Just to take an illustration: is light a force, or is it material substance? If it be a force distinct from matter, then is light an intelligent existence—is it mind? Now let us view light under the only two hypotheses we have as to its nature. Mr. Warington spoke of light passing through a vacuum. If so, what passes through a vacuum—that is, through space void of matter? Is it matter or force? Upon the emission theory of Newton, light is produced by the emission of matter called luminous matter—matter imponderable, and therefore not subject to the laws of gravitation. This matter can be projected through a vacuum, but not by itself. Of itself it is inert; it cannot move itself; or, if once in motion, it cannot change its motion. That which moves it is force, something essentially distinct from the luminous matter itself. Now take the undulatory theory. Here we can have no propagation of light through a vacuum. Light can only be propagated through a *plenum* filled with what is called a luminiferous ether. Light has been called a shiver or vibration passing through this luminiferous ether. But is not this ether, if such exist, matter? Can it shiver of itself? Something must cause it to vibrate which is not matter, and which is force. Is this something necessarily mind? Now we cannot take this single instance into consideration without seeing how soon we are led up from matter to something higher than matter: to something capable of acting on or controlling matter, which is not matter, and which we call force. Who can tell how many different kinds of force are to be found in nature? Matter also may have force inseparably bound up as it were with its existence. We can conceive every particle of gold or silver having many such forces inseparably united with it. The forces of gravitation, molecular and chemical forces; forces which make particles of gold and silver combine with one another, or different particles of other material substances according to many laws, of the majority of which we are most likely still ignorant. These forces we may conceive indissolubly united by the Creator with the particles of gold or iron at their creation. Such forces, however, I cannot conceive to be intelligent existences. Nor are they the only forces existent in nature. There are higher forces capable of controlling these forces. I know no force existing, in gold for instance, capable of transferring every particle with which it comes in contact into gold. But if I take the tiniest living seed that ever grew, I find in it certain evidence for the existence of a force far different from the forces inherent or inseparably connected with dead matter. Whatever evidence I have for the existence of chemical or molecular forces in a particle of gold, an acorn

affords just as good evidence for a far higher class of force than these. A potential force, capable under certain circumstances of converting any given amount of certain kinds of matter into a forest of oaks of any given magnitude, and reproducing other acorns *ad infinitum*. This living force I may well conceive from its higher power of controlling the forces of dead, inert matter, as a force of a more powerful nature than these. But this living force, controlling the growth and structure of animate nature, leads us up to a higher force still—the force of intelligent and voluntary agents. Then, again, the mere instinctive intelligence of lower animate nature leads us up to the power and exertion of the will of intelligent agents like ourselves. But are we to stop here, on the confines, as it were, of the exposition of the existence of intelligent mind, which we know experimentally to be so powerful? Man, by the force of his intelligent will, can cause charcoal, saltpetre, and sulphur to combine with each other, and give him a compound with which he can rend asunder the strongest rock. He may tame the lightning, and make it whisper his message from the Old to the New World. Is this no miracle? Is this no invasion of the law of the conservation of material energy? Without the force of man's will actuating the material agents he controls, could these changes of material nature take place? Are there not human miracles the products of human minds? Could a microscope or a telescope be developed by any of the laws of inorganic nature from glass and brass, without the controlling interference of human thoughts, invention and skill? Force is the link, indeed, which binds the world of matter to the world of mind or thought. Each step we take from the forces of inorganic nature to those of animate structure, and from these upwards to the power of force produced by the intelligence of beings armed with the power of exerting free will, leads us up to forces of greater power and intensity. If this be so, are we to stop here? I maintain that such thoughts as these lead us upwards to the Great Power and Mind which is the Creator and Sustainer of all things; that if puny man has by the power and force of his mind an intelligence that can reach the furthest limits of the visible universe, an intelligence that can produce so much, an intelligence that can control so greatly the powers and forces of animate and inanimate nature; I can believe, without any sacrifice of philosophical thought or accuracy, that Almighty God, in answer to our feeble prayers, may indeed control the winds and the waves, and give rain and sunshine, fruitful seasons, and abundant harvests, filling our hearts with joy and gladness. Nay, more, He can work greater miracles than these. He can give us those supernatural graces by which alone our spiritual being can be fitted for an entrance into everlasting blessedness. (Hear, hear.)

MR. REDDIE.—As the issues under discussion are chiefly metaphysical, I should be very glad if a gentleman I see present, the Rev. Mr. Greig, would favour us with some observations on a matter he is so well qualified to discuss.

REV. DAVID GREIG.—My Lord, Mr. Reddie seems anxious that I should say a few words on the paper. This I shall gladly do; but I fear it will be

to little purpose. Although my friends sometimes give me credit for metaphysics, I cannot speak offhand upon that subject, and if I attempted it I fear I should not be intelligible. There is one thing, however, I would wish to say, and that is, that I am very much struck by the value of the paper which has been read to us. (Hear, hear.) I think it exactly meets the great difficulties with which religious matters have been surrounded at the present day. These difficulties I have never regarded as scientific, properly so called : they are metaphysical or philosophical ones. And this paper appears to me to state that philosophical view which is in accordance with Divine revelation, as opposed to that philosophical view adopted by a certain class of scientific men which is opposed to revelation. Mr. Warington has criticized the paper upon a good many points, and it is my misfortune to feel that those points which Mr. Warington has called in question are the very points which I admire most. (Hear, hear.) I am sorry for that. If we take, for instance, the discussion with reference to Mr. Mill's doctrine, which centres in the word "know," the whole point of the question, as between the two philosophies, is summed up in this,—whether knowledge expresses an active power or a passive impression on the mind. If knowledge is simply an impression derived from the senses, I cannot see how you can avoid the conclusion of Mr. Mill,—that conclusion which was first drawn by Bishop Berkeley, with regard to the non-existence of the material world, and afterwards by Hume, with regard to the non-existence of the spiritual world. Mr. Warington appears to assume that all our knowledge is from the senses. If so, by what sense do we know material substance, or our own personal existence ? We cannot see the soul, nor hear it, nor feel it——

MR. WARINGTON.—I spoke of external matter.

REV. DAVID GREIG.—Take matter. You cannot feel the substance of matter, you cannot see it. All that you have by the eye is simply an impression of colour, by the hand is simply an impression of resistance, and so on. Now, if all our knowledge is from the senses, how, in these circumstances, are you ever to get beyond impressions ? It is impossible. An impression is just an impression : you cannot make anything else out of it. Thus, under this supposition, there is nothing in the world but impressions. You remove God and man and matter, leaving only a series of impressions. I do not see how you can avoid that conclusion. But we take our stand upon that which Professor Kirk has brought out. When we say we *know* a thing, we assume that there is something active in that knowledge. We assume that there is something in the mind which has the power of knowing. The process is this : We receive an impression from sense. The mind is at first buried, so to speak, in the impression, but immediately separates itself from it, sets the impression before it as an object, sits in judgment on it, and draws conclusions. In this way the mind arrives at the conclusion of the existence of a soul in man, and of the existence of an outer nature. (Hear, hear.) There is just one other point I would make an observation upon, the distinction between the laws of nature and the usages of nature. It is a point extremely difficult to make intelligible ; but there is a distinction in it,

and it is important. What I would regard as the laws of nature are simply those great forces, such as gravitation, heat, and the laws that govern chemical and organic nature. Now, these I would say are the laws of nature, and the individual events and things in the world are produced by combinations of these laws or forces, and these combinations, I understand, Professor Kirk would distinguish as "usages." That, I think, is an important distinction, because it will be found, as Professor Kirk has said, that there is no law of nature violated by the miracles of Scripture : only the usages of nature are affected by them——

REV. W. MITCHELL.—And so they are affected by free will existing in beings possessing perfect will, and continually interfering with the ordinary course of those laws.

REV. DAVID GREIG.—Now, the way in which I would conceive of Almighty God in His relation with nature would be as of a Supreme Personal Being, absolutely free, who can combine according to His will and pleasure the laws of nature. He does not violate His own laws, but combines them for the attainment of His great purposes in the kingdoms of nature and of grace. Just as man, who is a free agent within his limited sphere, can combine laws of nature to attain his ends ; so God, who is absolute and over all, combines His laws for His supreme providential purposes. Further, man is a personal being, and the only relation in which he can stand to God is a personal relation, just as we are in personal relations with each other. Now it will be found, that if you once grant that there is a personal being in man, and that he stands in a personal relation with God, you have granted the principle of miracles. (Hear, hear.) On the other hand, if you deny a personal being to man or to God, and adopt as your theory invariable sequence of events, it will be found that not only miracles but everything else which a man believes in is absurd. (Hear, hear.) I only wish to say further how much I admire the paper which has been read. It is a paper which deserves our best consideration.

THE PRESIDENT.—Ladies and gentlemen, I have to announce that from this night we adjourn until our next session ; and that interval, it is hoped, will be well employed by you and other members in endeavouring to extend the influence of our Association, and to secure new members. You see what a vigorous infant the Victoria Institute is. It is, indeed, an infant Hercules, and it has become so because it rests upon a true basis. I hope the influence of this Association will continue to extend. It seems to combine true vital Christianity with the largest adoption of true liberal science ; and I think we shall be enabled to show, by the agency of our members, such as my talented friend, Mr. Walter Mitchell, of whom I cannot speak with sufficient respect, that science and religion go hand in hand, the truth of both coming from the same God, and leading to the same grand destination for the human race. (Applause.)

REPLY BY PROFESSOR KIRK.

IN briefly replying to the remarks offered on my paper, I must, first of all, acknowledge the extreme kindness of the Honorary Secretary in doing greater justice to the essay than I could have done myself, and also the great kindness of the noble President and others in speaking of it as they have done. But I must specially thank Mr. Warington for giving occasion to a discussion every way gratifying to me, and for indirectly adding so much to the force of the argument which I have endeavoured to advance.

As to my long introduction, I must plead that it is only in metaphysical discussions that we meet with questions respecting the nature of knowledge. Chemists, for example, do not trouble themselves as to whether they really know the substances with which they experiment; nor do astronomers inquire whether they see the stars or only their own sensations when observing; but metaphysicians encounter such questions everywhere in their investigations and discussions; and little, indeed, can be understood in the relations of their science until we have somewhat settled ideas as to the nature of knowledge.

I must confess that I am rather astonished at Mr. Warington's remarks on what he calls my "scientific errors." As to light being "only a movement in the atmosphere," my words are—"The light is but a state of movement in the atmosphere:" that is *the* light of the lighthouse of which, in the words referred to, I am speaking, as a movement passing over many miles of ocean. Light is a movement of the substance which is illuminated: it passes through transparent solids and liquids as it passes through transparent air. As to its passing easily through a *vacuum*, that is a matter more easily asserted than proved. If Mr. Warington means by "a vacuum" a space from which air is excluded, while it is full of some other substance, his statement is no doubt true as he means it; but he will, I suspect, find it very difficult to secure a *real vacuum* by means of which to show how easily light passes through *it*. Should he mean to assert that light passes easily through a space which is empty of all matter I fear his *statement* is self-destructive—and that, too, whether we regard the light as a movement or as a substance. If it is merely a movement, it cannot be where there is nothing to move; and if it is a substance, that cannot be a *vacuum* where a substance is, even if only "passing through." The "erroneous idea" is in Mr. Warington's logic in this case; but I am confident that it is not "ingrained" there! Nor is the "ether," which he fancies, so "ingrained." My words in alluding to this are,—“the ether, imagined as filling up the spaces between the atoms of matter.” This is distinct enough from so-called "ether" which is supposed to exist in the spaces between the celestial bodies, and the positions of the two stand wide apart in philosophy. As far back as 1842, Grove said:—"It appears to

me that heat and light may be said to be affections, or, according to the undulatory theory, vibrations of matter itself, and not of a distinct ethereal fluid permeating it: these vibrations would be propagated just as sound is propagated by vibrations of wood, or as waves by water." Professor Grove quotes himself (as having used this language before such ideas were publicly advanced) in the preface to his great essay on "The Correlation of Physical Forces," which had reached its fourth edition in 1862. In that part of this essay in which the distinguished author treats of light, he says,—“Light was regarded by what was called the corpuscular theory, as being in itself matter, or a specific fluid emanating from luminous bodies, and producing the effects of sensation by impinging on the retina. This theory gave way to the undulatory one, which is generally adopted at the present day, and which regards light as resulting from the undulations of a specific fluid to which the name of ether has been given, which hypothetic fluid is supposed to pervade the universe and to permeate the pores of all bodies. In a lecture delivered in January, 1842, when I first publicly advanced the views advocated in this essay, I stated that it appeared to me more consistent with known facts to regard light as resulting from a vibration or motion of the molecules of matter itself, rather than from a specific ether pervading it.” Mr. Grove mentions Euler as having published a somewhat similar theory.* The arguments advanced by this philosopher, apart altogether from his name, more than warrant us in setting this “ether” down as a nonentity. At the best, besides, it had never more than a hypothetical existence.

It is not necessary that I should do more than notice Mr. Warington’s remarks on the nature of knowledge. My words on this point are to the effect that all our “impressions, outer and inner, are but the raw material, so to speak, from which knowledge is manufactured.” Mr. Warington comes to the conclusion that, “the impressions received by our senses constitute the whole amount of our knowledge—or (he adds), *to speak more accurately*, the materials for our knowledge.” So far, therefore, as he speaks “*accurately*,” Mr. Warington says just what I had said; and it would be hypercritical in me to deal with what are merely his acknowledged inaccuracies. Mr. Greig has spoken effectively on the passive and active views of knowledge, as argued in this part of my subject.

As to my remark regarding getting at *what we mean* by a *mode*, Mr. Warington mistakes me, as if I had spoken of getting at a concrete idea from an abstract, while I speak rather of how we *analyse an abstraction* which we have conceived vaguely. Having risen from the concrete too hastily—or having accepted the abstract at second hand—we need to go back in order to clear up our thinking.

Mr. Reddie has exploded the gunpowder element in the criticism; and I need only repeat that a cake from the interior of which atmospheric air is excluded, is surely a very different substance from a powder with which it is

* Grove’s Essay, Preface, p. xi., and pp. 162 and 163.

intimately mingled, and by which it has been *chemically affected* in being so mingled. There is, beyond all question, a substance in the powder which is not in the cake, as truly as there is one in proper gunpowder which is not in that which lacks sulphur.

The two pieces of iron—one magnetized and the other not—afford an illustration of a truth which Mr. Warington does not seem to have apprehended. The magnetic current is passing through the magnetized iron, as the current of heat passes through the proper gunpowder, when a red-hot wire is applied to it. The magnetic “affection” of this iron is the result of its having had the magnetic current introduced to it; just as the explosion of the gunpowder is the result of introducing the “affection” of heat from the wire. The iron which is not magnetized is simply a piece of that metal which has not yet been placed so as to receive the magnetic stream. It is like a portion of good gunpowder which has not yet been fired. It is consequently not in that state of magnetic agitation in which it would attract other pieces. But the instant it has the magnetic current introduced, it is affected, and affects in turn, like the other. We therefore argue that the substances are alike, inasmuch as they are both affected equally and made to affect other masses by that movement which we call magnetism. If two bars are placed equally in a magnetic current, and the one is magnetized, while the other is not, we inevitably conclude that there is something in the one which is not in the other. My argument is therefore perfect.

As to Mr. Warington’s defence of Professor Grove’s confounding “force” and “motion,” I have only to say that I think it is a hasty argument on behalf of loosely employed language. I certainly do not admit that “force is nothing more than motion,” any more than I admit that “cause” is nothing more than “effect;” and I must contend that so long as philosophers are content with that confusion of thought, and of words which mix up force with motion, cause with effect, and law with observed uniformity, they are not likely to enjoy the truth. But I have said enough as to this in the paper itself.

It is, perhaps, more important to speak of Mr. Warington’s idea that mind is only a *directive* cause. His own illustration of “the gas” ought to light him out of the notion. Because the pipe will not light when there is no gas in it—that is, because the gas will not light where it is not; because the gas must exist in order to be in a state of combustion—he argues that when there is gas in the pipe it is not the person who applies the match to it who is the cause of the light! He says, too, that the “combustion is the cause of the flame”! I humbly think that the flame consists of the gas and a portion of the atmospheric air in a state of combustion. The mere state of a thing cannot be the cause of that thing; nor can such state be its own cause. This state of combustion is communicated when a match in the same state is brought near enough to the combustible substance, as a ball in motion communicates its motion to a ball at rest when the one hits the other. It is certainly inaccurate to say that the combustion of the gas is the “cause” of the flame, even as Mr. Warington would have us to use

the word "cause." He might perhaps, in accordance with that use of the word, speak of the combustion in the match as the cause of the combustion in the gas, and that again loosely as the "cause" of the flame in the burner. But if he means to use language with philosophical accuracy, and to think clearly on the subject, he must use the word "cause" in the sense of *the first to move* in the series of motions in question. If he does this, and goes back till he finds out the *first mover* in the lighting of the gas, he will get beyond the "combustion" even of his own brain, so as to fix on that "combustion" which I call his *will*, whatever that may mean. He will find, in truth, that he himself is the responsible *originator* of his actions and their proper consequences, and not merely a *director*, as he imagines.

On the two points of "motives" in the world of mind, and "conditions" in that of matter, Mr. Warington seems to have but one leading idea—it is that "conditions determine the course of events." If I understand him aright, he means, with Mr. John Stuart Mill, that an "assemblage of conditions" is that which alone is properly regarded as a "cause," whether in relation to moral or physical occurrences. Mr. Warington's words are: "It is to be remembered, that a man's action is the result, not of one motive acting alone, but of a whole series of motives variously counterbalancing each other." This exactly expresses Mr. Mill's idea on the subject. The strictly logical effect of this notion is the belief that the universe is a *machine* whose purely *mechanical* movements embrace all those of mind as well as all those of matter. Professor Tyndall gives expression to the state of soul which *craves* this idea. In his article on Miracles in the *Fortnightly Review* for June, 1867, speaking of the relation between "forces" and "phenomena" as "necessary," he says,—“Not until this relation is established is the law of reason rendered concentric with the laws of nature, and not until this is effected does the mind of the scientific philosopher rest in peace.” That is, when put into plain words,—the mind that can rest in anything but the absolute and universally *mechanical* is not that of a "scientific philosopher"! If conditions *necessarily* determine results, so that all natural sequences are matters of pure *necessity*, then there is not only no man—there is no God that determines anything. The "conditions" arise as the *necessary result* of "conditions" that were necessary before them, and so on back to all eternity! So, too, must it be forward to all eternity! And is there no one who may be called a "scientific philosopher" who can rest in peace in any other view of the universe than this? Mr. Warington is very far indeed, I am sure, from entertaining such a view. He does not think out his ideas as Professor Tyndall has thought out his; but so far as he holds that "conditions determine results," and forgets the *personal will*, which alone is true cause, he is on the same track with the believers in a *mechanical universe*, and from whose belief the idea of the living God is effectually excluded.

But this mechanical theory is utterly inconsistent with that observed variation which Mr. Warington has not fully considered, and which is as assuredly a matter of scientific certainty as anything can possibly be. Mr.

Warington says that, "in cases that at first sight appear to have this kind of variability, the progress of science has shown that they are really subject to law; and so analogy would lead us to suspect the same thing in other quarters." He gives the winds as an example of that which has been found to be fixed by invariable law. But if it were true that the fixed laws of the winds had been discovered (as it is not true), that would be only a case of inorganic matter having invariable rules of motion when affected by the action of Him who gives that motion, and could have no such analogy to the laws of life as to lead even to the *suspicion* that these must be of the same nature. But surely Mr. Warington does not mean that we are to take "*suspicions*" for science. He cannot contend that a "*may be*," or that even a "*must be*" in the mind of a "philosopher," is to be set down as truth. *Variation is the law of all living organism*, so far as facts teach us. This is the result of discovery—a result so established as to lead to the idea, which I have noticed in the paper, that even man himself is but the last variety in the ever-varying universe. I may certainly say that, if science has taught us anything, it has taught us that variation is Nature's law of life.

Mr. Warington is dissatisfied with the distinction between "usage" and "law," and he seems to think that we depend on infractions of natural law for our belief of the supernatural. Even in such a case as that in which the dead arose at the command of Christ, he cannot see the supernatural but in the breach of law. But he means by law uniformity of occurrence, and nothing more. The "law" which he contemplates as violated in the miracle is nothing beyond this uniformity "*as known to us*." If he will think at all carefully he will soon see that this is really no law whatever. It is not even usage. One man has observed uniformity of occurrence only to such an extent that another man has observed variation in that which the first has observed to be uniform, and that second man has observed only so far that a third has observed variation in his uniformity, the third has been corrected by a fourth, and so on. A "*law*" to one generation is a "*fancy*" only to the next. A "*miracle*," in this sense, to one crowd, is only a natural transaction to another. It is such a "*law*" as Mr. Warington contemplates that "*like shall produce like*." A man observes this, for example, in a breed of certain animals, and he holds his observation to be that of "*a natural law*." Another man has had a wider field, or better opportunity of observation, and he has seen an instance of striking *unlikeness* in certain individuals among the produce of the herd. The "*natural law*" of the former man is seen to be "*violated*;" in other words, it is seen to be no law at all. No one thinks this so-called "*sport*" a miracle, nor can any one who knows what he is saying call it an infraction of law. It is a departure from observed uniformity—or, as I would say, a departure from usage—though no such departure as indicates the "supernatural." We may surely distinguish between that from which this is a departure, and those *laws* or principles of being itself, from which there can be no departure in the actions of God. Calling the dead to life by the human voice is a departure from usage, such as does indicate the supernatural; not

because it is the infraction of any of these laws, but because it is an act in which all that we call *Nature* is so distinctly shown to be at the will of Him who performs it. In truth *Nature* is only a name for that extremely partial idea which men have of the universe apart from God ; and no doubt such an act as this makes sad work of the idea. It is infraction enough of the essential elements of which such an idea is composed, though no infraction of any strictly divine law regulating divine action on either matter or mind.

Mr. Warington says that "there may be higher laws" than those uniformities which are called "usages." I think he may very safely say that there *are* such higher laws, especially when he is thinking of merely material uniformities as the lower. There *must* be higher laws than those which affect the lowest things in the universe ; or how are the higher existences to be ruled ? There must be moral laws as truly as there are moral beings. And we believe that one of the most momentous of these is that which was obeyed by Jesus before he called Lazarus from the grave, namely, when he *prayed to the Father*.

On the subject of the "infinite" Mr. Warington is, I think, in confusion, because he fails to distinguish between *measuring* and *conceiving*. His words are,—"*Can we conceive of an object having no limits ?*" I understand that he argues against the possibility of such a conception. But his argument is valid only against our *grasping* the infinite. He says that he believes in the infinite, but as to getting a measure of it we utterly fail. He seems to argue that, because we cannot get a measure of it, we cannot come into contact with it so as to conceive it as infinite. I cannot admit the validity of such reasoning. We come in contact with multitudes of things of which we have no measure, and that too so as to perceive that to us they are immeasurable. We clearly conceive their *immeasurableness*.

All will easily believe that I am far from delighted to find that I differ in idea from the conclusions of our excellent Vice-President, Mr. Mitchell. And I feel that I must say a few words in reference to that "force" which he believes to be a substance, and which is neither mind nor matter. It may be necessary to explain that I do not think *intelligence* essential to mind, when contemplating the great whole of immaterial being. The self-mover is mind, as I understand the word, whether capable of thought and emotion or incapable of these. If I take the lowest animal in the scale of life which is self-motive, it may be difficult, if not impossible, to predicate *thought* of that creature, yet it has what I understand in this discussion as a mind. The living seed to which Mr. Mitchell refers is not a substance of this nature. There is no force in that seed such as *originates* any change either in itself or in anything else. It is a mistake in philosophy to imagine that a seed exerts any force analogous to that which belongs to what we call mind, even as that is found in the lowest animal. The seed, when placed in the current of certain motions, is put in motion and kept in that peculiar state of agitation in which it is developed and increased as a piece of any other mere matter is developed and increased when brought into contiguous agitations. We know

that the seed does not do any one thing *of itself* as the animal does. We call the changes through which it passes *life*, because they resemble the changes of the truly living creature more nearly than those of inorganic matter, but there is nothing in any or all of these changes of the nature of that self-moving or self-acting which is observed in the animal. It is, I humbly think, in this *self-acting*, and not in intelligence, that we discover the essential quality of true mind. If there is a substance which may be called force, and which is neither matter nor mind, it must be something essentially distinct from all that is merely moved, and also from all that *wills*, or originates motion, in living entities having the power of *volition*. It must not be like the seed, which is only moved in the streams of agitations by which it is surrounded when placed where these agitations prevail; and it must not be like the force of will, which is the essential characteristic of the true mind, whether intelligent or non-intelligent. Can we form a conception of this substance for which so many philosophers contend, and of which a particular school make such an extravagant use?

Mr. Mitchell says truly, that "the purely physical reasoner has a distinct conception of force and matter as two very different existences." But may I not ask whether his conception of "force" is not in very many cases merely a conception of "motion," which he mistakes for force? Was it not this mistake which misled Boscovich and Faraday, and which misleads a host of such men as Professor Tyndall, who follow in the wake of original thinkers more readily in error than in truth? What Mr. Mitchell says of light may help us here. He asks whether it is "a force or a substance." It is neither the one nor the other, but simply *motion*. Were you to adopt the now abandoned idea of a luminiferous ether, it is the "shiver" of that ether which constitutes light. A shiver is not a force but a motion produced by a force.

As Mr. Mitchell rightly says, "Something must cause it (that is the ether) to vibrate, which is not matter and which is force. Is this something," he asks, "necessarily mind?" Let us see. We must leave out the "necessarily," as I am not trying to show *what must be* but only *what is*. Is the true cause of the agitation in a luminous substance actually mind? We shall have help here from Grove's "Correlation of Forces." I hold in my hand, we shall say, an ordinary match, and I stand amid perfect darkness; I bring the match into contact with a suitable surface. Here is *motion*, but not sufficient motion to issue in light. I draw the end of the match quickly over the surface with which I had brought it into contact, and this motion passes into heat, and that into all those other motions which issue almost instantly in that which illuminates. Now we have matter and motion in that instance—one mode of motion passing into another mode—and we have force causing this train of motions—but that force is nothing more or less than the force of *mind*. The conception of the physicist who confounds this force with the motion which it produces may be clear, but it is not correct; and we see the consequences of its incorrectness in the sad conclusions to which it leads those who follow it logically out.

It is held, I think, by all sound thinkers as well as by many that are unsound,

to be inconsistent with true philosophy, when we imagine a cause that is not required to explain all the phenomena in any particular case or class of cases in Nature. But if we imagine a force, in such a case as this of lighting the match, which is neither matter nor mind, we do imagine an unnecessary cause. Motion originated in volition by mind, passes on its course, changing from one mode into another till light appears. There is mind and matter—force of mind and motion of matter—but nothing more. If we rise from the lighting of a match to the kindling of the great sun itself, what reason can we have for interpolating a “force” in that case which is totally wanting in the other? If it is argued that though God kindles the sun, there must be a force or cause then, in the sun itself, such as makes the agitation in that orb go out into space, I reply that this agitation passes to all surrounding objects, as ordinary motion passes from one portion say of water to another, and it passes through all objects that are susceptible of such agitations; but this is essentially unlike that which is, I think, properly called “*force*,” as that exists in mind, *originating* motion, and accounting for its existence. We have mind and matter—the force of mind and the motion of matter—there; and true philosophy not only asks no more, but refuses to admit any more.

I am glad to see that all who have spoken on the subject see the importance of the metaphysics, or, as I should call it, the philosophy of this great question, and perceive that it is in this region that the difficulties of inquiring minds chiefly lie. It is consequently this same region which we must enter, to deal with those difficulties. In this work I have offered my humble share of effort in the essay in hand.

But I seem to have said enough, and will only add my very warmest acknowledgments of the kind manner in which I have been dealt with by all concerned.

ORDINARY MEETING, DECEMBER 16, 1867.

THE REV. WALTER MITCHELL, M.A., VICE-PRESIDENT, IN THE
CHAIR.

The minutes of the last meeting of the previous session having been read and confirmed, the names of the following new members and associates of the Institute were announced :—

MEMBERS for 1867 :—Charles Brooke, Esq., M.A., M.B., F.R.S., 16, Fitzroy Square ; William Brooke, Esq., A.B., Master in Chancery, Taney Hill, Dundrum, Dublin ; Horatio Darby, Esq., Hereford ; Charles Hammond, Esq., M.D., Bentley, Farnham, Hants ; William Hooley, Esq., Stockport ; John J. Jackson, Esq., Fern Cliff, St. John's Park, Blackheath ; The Honourable Sir Robert Lush, Judge of the Court of Queen's Bench, 60, Avenue Road, Regent's Park ; J. Pattison, Esq., M.D., 10, Cavendish Road, St. John's Wood ; Rev. J. H. Titcomb, M.A., Wingfield House, South Lambeth Road.

ASSOCIATE 2ND CLASS : Rev. David Greig, M.A., 45, Grove Place, Brompton.

MEMBERS for 1868 :—Henry W. Bleby, Esq., 88, Chancery Lane ; John T. Mould, Esq., F.R.C.S., Onslow Crescent, Brompton ; Rev. C. A. Row, M.A., 55, Gloucester Terrace, Regent's Park.

ASSOCIATE 2ND CLASS : Rev. Nicholas C. Martin, A.K.A.B., The Rectory, Carndonagh, Ireland.

The CHAIRMAN stated that Mr. Charles Brooke had been elected a Vice-President of the Institute, and it was expected that he would have occupied the chair, but had been prevented doing so by illness. He also announced, with a regret and sympathy which he was sure would be shared by the audience, that Mr. Reddie, who had written the paper for that evening, was also unable to be present, on account of the serious illness of his wife. Under those circumstances, the Rev. Dr. Thornton had kindly undertaken to read the paper for Mr. Reddie.

The Rev. Dr. THORNTON then read the following paper :—

ON GEOLOGICAL CHRONOLOGY, AND THE COGENCY OF THE ARGUMENTS BY WHICH SOME SCIENTIFIC DOCTRINES ARE SUPPORTED. (*In reply to Professor Huxley's Discourse delivered at Sion College, on Nov. 21st, 1867.*) By JAMES REDDIE, ESQ., HON. SEC., VICT. INST.

AS this Paper comes before the Institute under somewhat peculiar circumstances, I beg leave to occupy your attention with a few words of explanation.

So lately as a month ago it was utterly improbable that I should have written the first Paper of the present Session. It had even been settled not to commence our meetings till after Christmas ; and I myself suggested to the Council the desirableness of putting forward a programme of Papers entirely by new authors. I was therefore looking forward to a little rest, or the pleasure of only listening to Essays written by others. I shall only further premise that when I found it necessary unexpectedly to intrude this paper upon your notice, I begged for an extra night, so as to disturb our preceding arrangements as little as possible. I also asked for an early evening, because the matter that has forced me to write was one that did not brook delay. And I submit that if this Institute is to be of use with reference to those grave questions where science and Holy Scripture are alleged to be at issue—if, in short, the founding of the VICTORIA INSTITUTE was not a mistake—it is unquestionably our bounden duty to deal with the subject I am now about to bring before you.

PROFESSOR HUXLEY AT SION COLLEGE.

On 21st November Professor Huxley delivered an extempore Discourse in the hall of Sion College, the subject of which was announced in the following terms, in a printed circular issued

by the Rev. William Rogers, the President of the College:—"In opening the discussion on Thursday next, Professor Huxley will draw attention to the difference supposed to exist between scientific and clerical opinion, and inquire into the cogency of the arguments by which some scientific doctrines are supported."

The specific subject of the distinguished Professor's remarks was the evidence which he considered to be adducible,—from the civilization of Egypt at the time when the Hebrew Joseph was made Governor by Pharaoh,—from the Pyramids, and the mud-deposits of the Nile-valley,—from the nummulitic rocks and some other strata, and from the chalk formations,—against the chronology of *Genesis*. In other words, it was intended to be a brief summary, though certainly a new version, of "the testimony of the rocks," against what is popularly supposed to be the teaching of the Bible regarding the age of this world.

The greater part, however, of the Professor's address was occupied with an admonitory and apologetic exordium, followed by frequent subsequent remarks of the same kind, relating to the utter divergence he said there had grown up, and which he considered to be increasing, between what he called scientific and clerical opinion, or the habits of thought of the philosophers and the clergy of this country. This classification was questioned by several speakers and humorously criticised by some as "a rather strange division of the human race;" but as the learned Professor appears only to have adopted it *pro re nata*, as a tribute to the *genius loci* and while addressing "his hosts, the clergy," in Sion College, its propriety need not further be canvassed. From the discussion that followed, it was chiefly evident that the greater part of the Professor's address might have been spared, as it appeared to be founded upon a misapprehension of what really is the attitude of the clergy towards science; and so, we may give our attention rather to what he thought proper to say on behalf of himself, as representing the "men of science" or "philosophers." His professions of earnestness and honesty may be succinctly summed up in a noble sentence for which the meeting was indebted to Professor Tyndall, who afterwards spoke, and who told us, if I understood him aright, that it was a sentiment of Professor Huxley's own—namely, that he *would rather die than lie*. This is, I repeat, a noble sentiment, and it is one not more solemn than became the theme, when the issue, as it was then put forward, involves nothing less than the truth or falsehood of the Holy Scriptures.

As a layman, however, myself, I feel bound to say, on behalf of the Christian clergy, that this is surely a sentiment

which their predecessors in the faith have previously taught the world, and that not merely by precept and profession, but also in their persons by example, as confessors and martyrs for truth. Even if it be urged that there has sometimes been an unworthy exception, it may also be replied, there has been a glorious self-revenge,—as, for instance, in the case of Archbishop Cranmer, who thrust into the fire the hand that had signed a temporary recantation of what he had been persuaded was true. All men must admire such a spirit of self-immolation, whether holding Cranmer's opinions or not. On the scientific side, I must say, I neither know of such an "army of martyrs," nor of any such penitent heroism. Galileo, who is perhaps the most popular of the "martyrs of science," preferred "to lie" rather than to suffer or to die; and—unfortunately for his reputation—he preferred "to lie" most consciously, by profession and act and deed, for he did it the very moment before he meanly whispered to his friend, his notorious *E pur si muove!* On the other hand, Copernicus, who never thus disgraced himself, was an ecclesiastic; and his great work which propounded what he considered to be the truth relating to the universe, was, after he had suffered much on account of his opinions, and after he had been satirized upon the stage, actually given to the world at the instigation, and by the encouragement, of a cardinal of the Church of Rome.

But, in truth, to die for one's convictions, when that dire issue is forced upon men in the face of their fellow-men and before the world, is not the rarest of virtues; but whatever be its value, it is one of which "men of science" have had little or no experience. In this country absolutely none. There have been minor persecutions, no doubt, for the sake of science. I know those who have suffered them in England, even in these enlightened days; but they have not, so far as I remember, been encountered by the recognized professors of science. Davy, in his early days, and when opposing some scientific doctrines, was considered "a very troublesome fellow," and snubbed; not by the general public, however, or even by the clergy, but by a "professor" of chemistry.

I must not omit to notice here the once despised philosopher Socrates, a genuine martyr for truth and for freedom of thought. And who were his persecutors? The professors of his day, who pretended to know everything, and went about giving lectures and teaching for profit their deleterious sophisms. I trust such a state of things is not in store for us! Should it come, be assured we shall want our Socrates Redivivus!

Martyrs have often died for truth; but let us not forget there have also been martyrs of delusion all over the world. A higher and surer test of honour and of nobleness, a better proof of honesty in man, must be looked for, and can only be found, in his every-day, straightforward candour towards those from whom he differs, and in the patience with which he bears neglect, misrepresentation, or even contempt. It is also to be seen in the openness with which a man fights chivalrously under his true colours, and the frankness with which he makes admissions, when, instead of having always been right, he knows he has often been wrong. And, indeed, upon the whole, in England, men who honestly have acted thus, have generally been duly respected. In our own day we have seen two brothers, both highly distinguished in their university, one leaving the Church of England for the Church of Rome, the other renouncing Christianity altogether; and yet, though both have written bitterly against and ridiculed what they have repudiated, with all the earnestness of eager converts to new opinions, they are generally honoured and respected, and even sympathized with, by those who in controversy have been their uncompromising opponents. And this is what ever ought to be. If the names of others who have also changed their views, and denounced their former professions, have been held in less respect by their fellow-men, it is not because of their changes of opinion, or for the plainness with which they have spoken or written, but entirely upon other grounds, which I need not now particularize.

I am sure that Professor Huxley needed not, in order to satisfy the clergy or any other honourable and fairly-educated class of the community, to make the least apology for speaking fully and fairly his convictions. I am quite sure the clergy as a body are as free from what was styled a "sort of conventional dishonesty of society," as any other class amongst us. And I venture to think that it was an unfortunate error on Professor Huxley's part—though it was explained to have been done for courtesy, and in order not to offend prejudice—that he failed to speak all he thought bearing on the subjects to which he called attention. Where he spoke plainest I feel certain he gave least offence; while his hinted reticence of expression and assumed moderation—as if something dreadful were kept back—only served to give an almost intolerable air of patronage to his tone, and converted what every one could see were intended to be his arguments, into a sorites of insinuations.

After these remarks, I need scarcely add, that on the present occasion I intend to use all plainness of speech,

though to speak with all due courtesy; and while I shall keep nothing back essential to my argument, I shall make no insinuations either that I might have said more, or that others mean more than they have said. I shall try to meet the issues fairly; and I shall now begin by saying what those issues are. In the first place it must not be supposed that I am about to attempt to establish the truth of the Bible chronology, or even to state what the Bible chronology is. What I have written is "in reply to Professor Huxley." The subject is strictly an inquiry into the cogency of the arguments *he* adduced in support of some doctrines of geological chronology which *he* considers to be scientific, and which *he said* are contrary to the Bible chronology. I shall simply follow his line of argument, with the view of showing chiefly, without implying intentional unfairness, that he did not place the issues, nor even the facts that bear upon those issues, fully or fairly before his audience; also that his arguments were loose instead of being cogent, and that sometimes they were self-contradictory; and that, therefore, he did not succeed in upsetting the chronology of *Genesis* as interpreted by himself. If besides this I happen to make out a *primâ facie* case in favour of the particular Scriptural chronology which Professor Huxley denied to be true; and if the doctrines of geological chronology which he professed to believe are shown to be utterly disentitled to the term "scientific" in any sense; or if men of science are proved to be at issue about those doctrines;—all that will be more than might be demanded in a reply that will not go unnecessarily beyond the line of the arguments which had been advanced to establish the very opposite conclusions. For a fuller consideration of the various arguments, *pro* and *con*, relating to this great subject, I must refer you to some former papers in our *Journal of Transactions*, but especially to the comprehensive discourse upon "The Past and Present Relations of Geological Science to the Sacred Scriptures,"* by Professor Kirk. It could not be expected—as I ventured to tell Professor Huxley in Sion College—that the large issues involved could be satisfactorily disposed of in a single unreported discussion arising upon an extempore address. Nor, of course, can they be disposed of in this reply. Fortunately for my line of argument, I do not think that much, if anything, will depend upon nice verbal accuracy as regards Professor Huxley's statements; but, fortunately also, in case that might be thought of importance, a gentleman who took down the principal parts of Professor

* *Journal of Transactions*, vol. i. p. 331, *et seq.*

Huxley's discourse at the time, having seen in the *Record* newspaper a letter from me to the President of Sion College on this subject,* has kindly forwarded to me his notes, and of these I have gladly availed myself.

THE TEACHING OF THE CLERGY.

Professor Huxley, having finished his exordium, thus opened the issues of discussion :—

"You [the clergy] tell your congregations that the world was made 6,000 years ago, in six days, and that all living animals were made within that period," &c.

Then he added :—

"I am bound to say, I do not believe these statements you make and teach ; and I am further bound to say that I cannot call up to mind amongst men of science and research, and truthful men, *one* who believes those things, but, on the other hand, who does not believe the exact contrary."

Now, even here, without going further, I must ask, Is the Professor's statement accurate? Is it true that there is, or ever has been, such a uniformity of opinion among the clergy or other students of Scripture as regards the chronology of *Genesis*? Surely he knows something of the literature of the other side. Discarding altogether the interpretation now held by very many (as was stated by the Rev. Simcox Lea, in Sion College), namely, that the first verse of *Genesis* probably relates to a time at an immense chronological distance from the verses that follow; discarding also other modern interpretations, such as those of Mr. Rorison, Professor Challis, Dr. McCosh, and others, it is surely a well-known fact, that long before there were supposed to be any difficulties with science as regards this popular chronology of the Bible, the "days" of creation were by many interpreted as signifying lengthened periods, and not literal days of twenty-four hours. It is also a fact that the Hebrew, Samaritan, and Septuagint versions of *Genesis* all differ, as regards the chronologies of Chapters v. and xi.; and many chronologers would be found to give about 8,000 years, as more probably the age of the world as literally deducible from *Genesis*, than the 6,000 years of the Vulgate and Archbishop Usher. I am quite aware that 2,000 years is of little account in "geological chronology," as set forth by Professor Huxley; but then such a period might be of conse-

* *Vide* Note A, p. 370.

quence to the other side. If we will only think soberly as to what is now being discovered by the Palestine explorations, to remind us how much may happen, in much less than 2,000 years, to change the face of a country and bury its massive structures deep under ground ;—if we will think of the West Indian hurricane two months ago, and the earthquakes since, also of the recent East Indian cyclone and of Vesuvius as it is in eruption now ; and if we will pay but the slightest attention to the innumerable historical records of still more destructive cataclysms, by water, wind and fire, during the present era, and even within a few generations, we shall be all the better prepared to think wisely as to the overwhelming power of nature to transform the face of the earth, and to estimate more truly the value of time in a *non-uniformitarian* world, subject to such marvellous changes as we know to have been accomplished within the historical period, and almost under our eyes.

But we have now to examine into the implied agreement among the clergy in holding to the 6,000 years of the vulgar era. So far is it from being true that there has been this agreement, that Mr. Goodwin, in the *Essays and Reviews*, actually pointed scornfully to the variety of conflicting opinions, and to “the trenchant way in which the theological geologists” (as he called them) “overthrow one another’s conclusions.” So notorious is the difference of opinion that has prevailed as to this, not merely among the clergy, but among all who instinctively cling to the notion that the Bible is true, while still inclined to follow the teachings of human science, that in the valuable paper read by Mr. Warington* at the first ordinary meeting of this Institute, he pointed out that, not only was it not settled among theologians what was understood by the word “day,” but, with an extreme impartiality, he described the defenders of Scripture—not as bigoted and serried in prejudice and all of one mind—but as “a motley and discordant set, at war among themselves as fiercely as with the enemy.” I quote this strong language to show, that we are not afraid of plain speaking in this Society. We think the truth should be spoken—the whole truth, and nothing but the truth ;—because truth alone will last. Mr. Warington also pointed out, that as the arguments of some of the defenders of Scripture are mutually destructive, “a proportion of them must be wrong, and that the defence they make is, therefore, a source of weakness, and not of strength.” He goes on :—

* *Journal of Transactions*, vol. i. p. 85, *et seq.*

"It behoves the advocates of Scripture to consider this well."—I venture to interpolate that *both* sides should keep it in mind.—"We hear much now-a-days [he continues] of the contradictory hypotheses of science, and of the constant flux of opinions in the scientific world. . . . But are there no contradictory hypotheses among the defenders of Scripture? Is there no flux of opinion in orthodox views? . . . Ay, truly, and that to a far greater degree, and of a kind far more inexcusable. Does the gradual unfolding of new facts cause scientific theories to be perpetually changing, and allow, for the time being, of the existence of many conflicting hypotheses? Well, be it remembered that every one of these theories and hypotheses has its advocates and representatives also among the defenders of Scripture." (p. 100.)

This, you will observe, is a very different state of things to that described by Professor Huxley. Which is the true description? Some may think Mr. Warington was rather hard upon the defenders of Scripture, among whom, no doubt, the great body of the clergy will be found. It may be thought that it is somewhat unkind now, to reproduce such a graphic picture of "a house divided against itself." But, let me ask, Is it not well to know the truth? And will it not also be profitable, if this may help us to discover the great cause of these disagreements, and to trace the main source of this internecine war among the defenders of Scripture? Well, then, we have this well explained, in few words and in popular language, in the *Saturday Review* of 30th November last:—

"Professor Huxley and Professor Tyndall after him" (says the Reviewer) "were exceedingly cogent in their demonstration that, if science and the clergy are to get on together, the clergy must take their scientific facts from science. But the truth is, this is just what they do already."

This, you will perceive, quite agrees with what Mr. Warington says, as to the various conflicting hypotheses and perpetually changing scientific theories having found but too ready acceptance among the defenders of Scripture, and tempted them to these varying interpretations. But the Reviewer—almost unconscious of the importance of his reproving words—also says this:—

"However ludicrous the readiness of the clerical mind to accept such conciliations may seem; however absurd it may be in men to find rest, now in a gap between two verses, now in the hypothesis of visions, and now in a theory of pure poetry, the readiness certainly does not prove any attitude of determined hostility towards science; . . . The clergy, in fact, float along with the stream of general opinion, and, considering the necessary

hitches, it is no discredit to them if now and then they float a little slower than other people. . . . When Professor Huxley holds one view about the number of centres of human origin, and rival professors hold another, it is open to the general public to advance a third if it likes ; but when all the professors in the world announce a certain order of geological succession, the general public simply hears and believes."

It will now be evident why I have quoted from Mr. Warington and the *Saturday Review*, to correct Professor Huxley's statement. It is, that such of the clergy, and any others, who have been led by scientific theorists into holding conflicting hypotheses about the creation, may recognise whom they have to thank for inducing them to adopt what are now sarcastically styled only "ludicrous" and "absurd" interpretations. But seeing that all these variable opinions exist, the next question is, which interpretation ought I to defend in replying to Professor Huxley? My answer is very simple—I trust it will not startle "the clergy" who may be present this evening:—I must defend what Professor Huxley attacked. If my reply is to be cogent, it must go to prove that Professor Huxley did not succeed in discrediting the 6,000 years of Usher, which alone he argued against. It would not really be fair to meet the Professor's arguments with a profession of faith in periods as elongated and indefinite as his own. If I could do no better than that, I might as well astonish you, by saying with the *Saturday Review*,—"The lecture was admirable, the illustrations perfect, "the argument conclusive, and, unluckily, there is no one to "argue with!"—But let us now proceed to examine the Professor's first argument.

THE ARGUMENT FROM THE CIVILIZATION OF EGYPT IN JOSEPH'S TIME.

As it was the first time that Professor Huxley had addressed a body of the clergy, he said "he would therefore deal with "the subject in their own familiar method. He would take a "text, and give them a scientific 'exegesis' drawn from the "text." He selected this passage from *Genesis* (ch. xli., verses 42, 43)—"*And Pharaoh took off his ring from his hand, and put it upon Joseph's hand, and arrayed him in vestures of fine linen, and put a gold chain about his neck ; and he made him to ride in the second chariot which he had.*"

"Now I ask you (said the Professor) to depict to yourselves that marvellous valley of the Nile, where these events took place 1,800 B.C. No doubt the passage is historical; that is to say, that the Pharaoh therein spoken of, who had at his disposal so great wealth, and who was master of the civilization of the world at that time, thought fit to elevate one of his slaves, invest him with symbols of authority, and make him to ride in the second chariot of the land,—placed him in position, power, and authority next to himself. These things indicate great advances in civilization, and refinement, and luxury. Certain monuments of that era show horse chariots sculptured upon them, as in Joseph's time, when there must have been a great civilization. Before that there existed a people highly civilized, but with whom are no traces of chariots or domestic horses: thus we suppose a great interval elapsed. Now, when we examine the records of the past, more than 2,000 years before the Christian epoch, we find at Memphis, in the oldest pyramids, records indicating the high cultivation which existed then as now by the overflow of the Nile," &c.

He afterwards quotes Herodotus as saying—

"that this Nile valley was once a great arm of the sea, filled up in process of time by mud brought down by the Nile—this great Nile valley, 1,200 miles long—filled up by mud forced down the Nile. And unless you are prepared to deny this condition of things, that in the time of Joseph, and long before, this Nile valley must have been essentially what it is now, ask yourselves what period of time this process of filling up this huge arm of the sea must have taken."

In order to bring in this last allusion to the time of Joseph, I have extended this quotation beyond what strictly belongs to the present branch of our inquiry. But having done so, I feel some difficulty in commenting upon the strange matter it contains. I would fain copy from the moderation of Professor Huxley, when his "courtesy" (says the *Saturday Reviewer*) "became almost distressing as his sense of truth forced him to unroll the long series of geological formations which had preceded the chalk." Only, I require all the courteous moderation I can command, to contract and roll up again, into rational and actual dimensions, the Professor's extraordinary extension of the land of Egypt, and the stretching of all that Herodotus has said, or could have conceived, about the valley of the Nile. The whole of Egypt, as well described by Herodotus, from the city of Elephantine to the sea, extends only from about 24° to 31° 30' N. Lat., *i.e.* to less than 8 degrees, or about 480 miles! And instead of Herodotus dreaming that "1,200 miles of the valley of the Nile" was ever "an arm of the sea," what he distinctly says is, that *the space between the mountains below Memphis* seems to him to have been formerly "a bay of the sea" (*Euter.* ii. 10); or, as in another passage, "the land below Lake Mœris," and perhaps a little above it (*Ib.* ii. 4); and in another place, "a bay extending southward, and approaching, per-

haps, so as to meet each other, and to overlap to some small extent the Arabian Gulf" (*Ib.* ii. 11). Now, probably the whole extent of country that Herodotus intended to indicate was not a hundred miles in length, being merely the Delta and the flat region round about Heliopolis and below Memphis; and even if we measure from the position of the artificial Lake Mœris, and suppose that the head of the Arabian Gulf did not formerly extend north of 30° , still the whole length of the district indicated would be considerably under 200 miles. It is almost absurd to suppose that Herodotus imagined the mountains between Lake Mœris and Memphis, and those on the other side of the river were part of this "bay of the sea;" his whole language evidently referring to the Delta and the low alluvial flats "between the mountains." Thus he says, "for the Delta, as the Egyptians themselves acknowledge, and as I think, *is alluvial*, and (if I may so express myself) *has lately come to light*,"—meaning, as "land acquired by the Egyptians, and "a gift from the river" (*Ib.* ii. 15 and 5). Again, one of the reasons he gives for crediting this opinion is, that "Egypt projects beyond the adjoining land" (*Ib.* ii. 12). Now, any one may see, by a glance at a map, that the extent to which Egypt does so project is not half the length of the Delta, or less than 60 miles. What, then, to make of Professor Huxley's imaginary long "arm" of "1,200 miles," I am at a loss to know. It is just about ten times longer than any "bay" which Herodotus can have conceived; and fond of high figures as the ancient Egyptians were, (like some now among ourselves!) I am very sure that the learned Professor did not get any hint of his modern measurement of the Egyptian Nile-valley in the pyramid-records of those old "land measurers" who founded Memphis!

Then as regards the pyramids themselves, he spoke of them as built more than 2,000 years before the Christian epoch, or about 300 years before Joseph's time, and 200 before the time he himself assigned to Joseph; whereas the usual chronology makes the pyramids 200 years *after* Joseph's time, or 1,500 B.C. As I do not know where he gets these unusual dates, I shall only further observe, that although the founding of Memphis is given by some as in 2,188 B.C., the building of the pyramids is generally given as 700 years later, or B.C. 1,492.

But the principal argument relied on, in this part of Professor Huxley's discourse, was the evidence of great civilization in the text he quoted, and the supposed long time required for the attainment of this condition, but especially before chariots could have been invented by the Egyptians. As,

however, he very plainly said, that this great lapse of time was merely supposed, there is here no argument to examine. But it would have been well, if he had given the supposed dates of the two classes of sculptured monuments from which he derived his negative proof of the non-chariot period in Egypt. If found in the Memphis monuments "2,000 years B.C.," *i.e.* at the time the city was founded, and it being admitted to be history that in Joseph's time there were chariots, then the "supposed great interval," that it is assumed must have elapsed, is not really so great after all,—certainly less than 300 years, even if we further suppose that chariots were just invented at the time when Joseph was made governor; which is not probable.

As to the argument that the Egyptians were without domestic horses at the time when no chariots are represented in their sculptures, I will only say, that if we adopt the usual genealogy of the Egyptians as being the descendants of Mizraim, the grandson of Noah and the founder of Memphis, then we can scarcely imagine them to have ever been ignorant of the use of horses. But as to this, and also as regards the great advance supposed to be made by them in civilization when they built their chariots, I would suggest that the simple explanation of the meagre facts upon which all this speculation is based, may be, that the tribe of Mizraim did not find carriage-roads ready-made in the valley of the Nile when they founded the colony of Egypt! Hence the very natural delay that may have occurred before they introduced chariots after building Memphis. To us who are accustomed to read in earlier chapters of *Genesis*, of earlier periods still in man's history, and of his primal condition as being one of high elevation and of great capacity, the early civilization of Egypt presents no difficulties. In *Genesis* chap. iv. we are told that Cain, the very first man born in the world, built the city he called Enoch after his son; and we read then of men who handled the harp and organ, and of artificers in brass and iron. In *Genesis* chap. vi. we also read of the ark of Noah, a hundred years before the building of Babel, nearly 1,000 years before the Egyptian pyramids. And we know from the modern science of ship-building, and the proportions given for Noah's ark, that its construction bears testimony to a marvellous knowledge of mechanical principles, far exceeding any amount of skill required for the construction of chariots.

In homely phrase, "the cart is put before the horse" throughout this argument, deduced from the civilization of Egypt. Whatever we may think of the theory of development in organic life, or of "the number of centres of human

origin," Professor Huxley cannot be at issue with us on that account with respect to Egypt. We have certainly to account for the chariots there, but we have neither to wait for the development of the horses nor of the men! The Egyptians were clearly immigrants, attracted to the fertile valley of the Nile, *after*, we may presume, its supposed recovery from the sea—that is, if it be not maintained that the muddy-looking Egyptians suddenly started from the Nile-mud itself; for the only other alternative would be, that they were "sea-born" like the fabled Venus!—But, if immigrants or colonists, what becomes of the gratuitous assumption of enormous time for their civilization? The whole cogency of the argument will depend upon the condition of the tribe of Mizraim when they colonized the Nile-valley. And surely the men who at once proceeded to build Memphis would have been able then to make chariots; and if they did not, we may believe they only sensibly waited till they had constructed tolerable roads for them to run on.

But let us take an illustration as to this, from a state of things of which we have certain knowledge. Let us suppose some grand convulsion of nature to affect Australia, analogous to that which may have raised the nummulitic rocks about Egypt, from the bottom of the ocean, where they were no doubt prepared and formed. Let Australia be cast into the sea or submerged, for some generations, and in process of time raised up again above the waters. And then suppose some future archaeological geologist to discover there the evidences of the savage condition of the aborigines, as well as of the civilized colonists, side by side, or, merely in the cities of the latter, the traces of their early and their existing condition. What speculations might not then be indulged in, what unlimited drafts upon time might not be devised, to account for the great advancement in civilization and refinement and luxury in Australia, upon the theory that its present civilization had a savage origin!

But then the cogency of the argument would all depend upon that assumed theory being true. And, I will say this, that if man was originally a savage, or a speechless non-descript animal somewhat lower, (which we know is, or was, Professor Huxley's own opinion as published to the world not many years ago,) then I think the learned Professor will require considerably more time than he hinted would be necessary, and infinitely more than the facts and dates, as he stated them, can possibly furnish him, to account for the civilization of Egypt. He or we, it seems, are as yet at liberty to indulge in our respective views upon this point, if we like. But

I do contend, that in bringing these matters before the public in popular lectures, the real state of the question should be made known. Professor Huxley spoke in the name of science and of men of science; and he left it to be inferred that there were not two opinions as regards the doctrines he put before the clergy in Sion College. Now, I am obliged to ask, whether that is true? And I venture to say—though I trust that truth in science is not to be settled by majorities—that not even a majority of those who are reputed to be men of science hold the same opinions as Professor Huxley, as to man's origin or his advancement to civilization. At the British Association, in 1865—

“Professor Rawlinson publicly protested against the assumption that human beings were originally in that poor and destitute condition which had been described, and that they all rose from a state of barbarism. He held the very opposite opinion, viz. that they were created in a state of considerable civilization, and that while most of the races had declined into absolute barbarism, some races had never done so. The Egyptians, Babylonians, and Jews had never so declined.”

You will observe I am not asking your assent to Professor Rawlinson's views, any more than to Professor Huxley's; but only endeavouring to show that you ought not to accept as “Scientific Doctrine” all that has been professedly put forth as such at Sion College. I do not know whether you will consider that the doctrines there professed, so far as we have yet examined them, were supported by cogent arguments or not. But at any rate you must reject, as not a fact, that fanciful “huge arm of the sea” 1,200 miles in length; as being a stretch far up the river Nile, nearly three times beyond the whole length of Egypt; and as a notion not imagined by the acute Greek “Father of history,” or dreamt of in the days of the Hebrew, Joseph.

You must remember also that the argument, that a long time must have elapsed after Memphis was built before its founders advanced to build chariots, is entirely based upon a mere assumption, which is not yet accepted by the most credulous, as a “scientific doctrine,” and which indeed is self-destructive of their faith in the fact they argue from, namely, the existence of Memphis itself.—And now let us go on to the Professor's second position.

THE ARGUMENT FROM THE MUD DEPOSITS IN THE VALLEY OF THE NILE.

It was perhaps because it was here that Professor Huxley intended to found one of his strongest points against the

6,000 years of *Genesis*, that he did not pay sufficient attention to the geography, topography or geodesy of Egypt, and gave but a weak "exegesis" of Herodotus. Let us therefore give all the more careful attention to his argument from the Nile-mud deposits. This mud deposit, he said, was very old, older than the pyramids which he said were built upon it; and in order that those who heard him might never forget this assertion, he thought it proper to anticipate (very properly only to ridicule) the objection, should any one advance it, that the mud might have been afterwards put under the pyramids, instead of their being built upon it! But the only objection he heard from the clergy was, that he was wrong in his statement that the pyramids stood on the mud! He was told they were built upon rock, when he only ventured to suggest that they stood "upon rock *and sand*." But he added that it did not signify to his argument upon what they stood, as he only wished to prove, from the Nile-mud being older than the pyramids, what a long period must have elapsed before Joseph's time and before the pyramids of Egypt were built. He said:—

"These monuments,—built on the site of the Great Valley of the Nile, fertilized then as now by the deposits left by that overflow of the mud which became the source and cause of the land's fertility and produce,—these monuments evidently existed *after* this great deposit of mud, upon which they stand; and what is this Egyptian mud?"

Then follows the passage I have already quoted referring to the opinion of Herodotus. After which the Professor goes on:—

"Various estimates have been made as to the quantity of mud which is brought down year by year. I will rather understate than overstate the results. The general estimate of the process of filling gives *five inches in a century*. This no doubt is a correct estimate, but let us take the quantity to be 12 inches or 1 foot in every century, so that there may be no room for cavil. Borings were made, and it was found that in the valley of the Nile we could bore to 70 feet through this Nile-mud. Now 70 feet at 1 foot for every 100 years gives at once 7,000 years, a longer period than has elapsed according to the received chronology of only 6,000 years since the creation of the world."

Now, I think we may well object to this average for the Nile-deposits of 1 foot in a century, for two very cogent reasons, and not because inclined to cavil. Because (1st), if five inches is the correct and general estimate, it ought on that account alone to be preferred; and (2nd), because the one foot

in a century is incredible, and upsets Professor Huxley's own arguments. Let us deal with the last objection first.

Unless then the Professor is prepared to adhere to the position that all this "70 feet of mud" was deposited prior to the founding of Memphis and the building of the pyramids; unless he will now admit that it was not all deposited 2,000 years B.C., then we must clear away no less than 3,800 years' deposit,—that is 20 feet before the Christian era and 18 feet since, together 38 feet,—or considerably more than half the depth of the whole existing deposit, in order to know what the valley of the Nile was like at the founding of Memphis.

But prepared as we might be upon reflection to reject such an extravagant estimate, as almost tantamount to clearing out the Nile valley altogether, and leaving no sufficient extent of well-watered alluvial plain remaining, that would have been worthy of attracting the descendants of Ham to settle there; we must not forget that this argument is based upon the fact that the Nile deposit is going on still; so that, whatever be the true rate of deposit, we *must* clear away what was deposited from the days of Mizraim and Memphis to our own. Let us therefore now, in the second place, take what Professor Huxley calls the general and "correct" estimate of five inches in a century; and let us then see "the results." The deposit in 38 centuries, at 5 inches in a century, would give 190 inches, or 15 feet 10 inches, which must be taken off from the whole upper surface of the Nile valley, in order to know something of what it was like when Memphis was built. With this Herculean labour before us, it is well that we have been able to reduce the superficial dimensions of the length of the valley of the Nile to something less than 1,200 miles! But the whole breadth of the Nile valley at Heliopolis, *i.e.*, about eight miles above the apex of the Delta, is only some sixteen miles; and at Memphis it is but five. At both these places "borings" have been made; and one of them was certainly said to be 70 feet deep,—or rather it was 72 feet;—but that was in the deepest part of the valley—assuming water to find its level—within 200 metres of the river itself! But what of all the other borings, as to which Professor Huxley was silent? As the case was put at Sion College, you have to imagine an enormously extended valley, 1,200 miles long, and nothing less than 70 feet deep, filled up to the brim with mud! The conception is truly sublime, and on the largest scale. In comparison with it the real facts are almost contemptible. But we are bound to deal with the facts. Let me cite them from a small work by Archdeacon Pratt of Calcutta, that it may be known that all the teachings of Sion College must not

count universally on being accepted by the clergy! The Archdeacon observes, "The thickness of the Nile mud is very different in the several excavations in the same neighbourhood."* At spots 3,100, 784, and 1,215 yards from the obelisk at Heliopolis, and having different bearings from it, the thicknesses were found to vary from *less than 7 feet to upwards of 14 feet!* The precise measurements he quotes are 9·92, 13·25, 14·25, 14·8, and 6·67 feet, and they are taken from Mr. Leonard Horner's memoir in the *Philosophical Transactions* for 1855, pp. 132-136. In the borings made westward from Heliopolis towards the centre of the valley the depth increased, and the excavations were made on a pretty large scale up to depths of 16 and even 24 feet; beyond that they were more literally "borings," and the mud was found to be 60 feet deep near the centre of the valley. The width of this deepest part I do not know; but I have cited enough, I think, to show that—as might have been supposed—the basin of the Nile valley is quite irregular in its surface, and slopes gradually on each side towards the centre or channel of the river. It must be evident therefore that if we take off 15 feet 10 inches deep of mud all along the upper surface, we must very greatly reduce the width of the valley from what it now is. But we must do this if we would know what it was like 2,000 B.C. The valley must then be narrowed at the edge near Heliopolis by some two or perhaps three miles, for no sounding within 3,100 yards of that city was deeper than $14\frac{1}{4}$ feet, and there the valley is very flat, just as it is described by Herodotus. We must correspondingly take off some two or three miles from the opposite or western side; and this will reduce the expanse of the valley at Heliopolis, or eight miles above the Delta from its present 16 miles to 10 or 12. Of course as the valley narrows towards Memphis it may be deeper and less shelving at its sides, and the clearing of $15\frac{1}{2}$ feet of the upper mud will make comparatively less difference there in the width of the valley. But still the difference will be very great.

Let us now consider another result that follows from the facts we are dealing with. If 5 inches deep of mud are now ascertained to be deposited in a century over the whole expanse of the Nile valley as it now is, when 16 miles wide at Heliopolis; then supposing the river to bring down no more mud now than it did when its width there was only ten or twelve miles; let me ask, Are we to be visited with the dreadful penalty of being considered *not* "scientific," if we say that, therefore, the deposit must have been much greater in depth

* *Scripture and Science not at Variance.* Fifth edition, p. 138.

(if there is any cogency in these arguments at all) at the time of Joseph, and deeper still 2,000 years B.C.? If it is argued that the *quantity* of mud which the Nile deposits in a century now, it must always have deposited in each preceding century; and if that is to be regarded as a cogent argument capable of giving a firm foundation to "scientific doctrine;" then, I say, this requires you also to admit (whenever you pay attention to the dimensions and the form of the basin in which the deposit is to be laid down,) that the *depths* of the deposit *must* vary greatly as we go back in time, and *must* have been very much greater in the long past than now. If that be so, we cannot concede, (as Professor Huxley required of the clergy at Sion College,) that "in the days of Joseph this Nile valley must have been essentially what it is now"! That there was then a fertile valley there, we may readily concede; for what else could have induced the tribe of Mizraim to settle on the banks of the Nile? But we *cannot* believe the valley was then so extensive, or that all its "70 feet deep" of mud was deposited 2,000 years before Christ. *Professor Huxley cannot believe that himself!* And he will find that if 5 inches of mud are now deposited in a century, and if merely the *same quantity* has for many centuries past been depositing, that this valley will rapidly narrow as he goes down, and he will soon come to the surface of the basin and channel of the river, with no fertile alluvium on its banks! When he comes to this, let him propound a theory, in accordance with *his* philosophy, that will account for the existence in that condition of the heaved-up and divided mountains or scooped-out rocks that form the basin of the Nile valley; or that will account for the river, that flows along for more than 1,200 miles from its still probably undiscovered sources. For my own part, in pursuing this inquiry I have been forced to think, that the fertile valley of the Nile must have had its beginning when the waters subsided after the great Deluge, and returned from covering the face of the earth, though since then probably the greater part of the Delta has been formed, and the valley of the Nile has continued to fill up and to increase in breadth. But I must object to the notion of its filling up uniformly at the rate of 1 foot in a century. The estimate is outrageously extravagant. Even that of 5 inches in a century, *as the present rate*, is more than we shall know what to make of, when the valley narrows as we descend in depth, and as we go back in time. I should rather be induced to accept the estimate of M. Rosiere of 2 inches and 3 lines in a century, that is, less than half the 5 inches announced by Professor Huxley as no

doubt the "correct" rate. But whatever be the rate we may think probable, judging from recent observations—and even as to this, let me observe, all men of science have certainly not come to one conclusion—very sure I am of this, there will be difficulty experienced in all our calculations from the depths of the past deposits, if we will measure them by those of the last 2,000 years, or even since the time of Herodotus. Sooner or later, as we clear away in our imaginations the surface of this mud-deposit, century after century, we shall approach to the end of the series, and find it a much harder task to conceive how the deposits began, than to count up their accumulations. It is fortunate for us that we are obliged by our reason to know, that even the Nile deposits must have had a beginning; so that we cannot be satisfied with a speculation that speedily runs itself out and ends in a beginning that is simply an utter blank.

Here I think I might quit the argument from the Nile mud, having shown it to be not one whit more cogent than that advanced from the monuments of Egypt. But I confess I am loath to omit some notice of what is to be found in old Herodotus, about the pyramids and the valley of the Nile, sufficient to have rendered impossible such arguments as we have been examining, in favour of such "scientific doctrines"!

Let us then see what may be learnt from the old Greek historian. Professor Huxley asserted that Herodotus says that the Nile valley was once a great arm of the sea filled up in the process of time by mud brought down by the Nile. This was put forward as if Herodotus had testified this, and as a fact which had been ascertained. But that is not the case. What he says is, that the priests informed him that the greater part of the country had been acquired by the Egyptians (from the sea); which he says *appeared* to him to be the case; and his reasons for this opinion are worth attending to. He says—

"I therefore both give credit to those who relate these things concerning Egypt, and am myself persuaded of their truth, when I see that Egypt projects beyond the adjoining land, and that shells are found on the mountains,"* &c.

I need quote no further here, because you will observe Herodotus has already proved more than enough. He has proved, not that *the valley* had been under the sea, but *the mountains* that form its basin; though he probably was not

* *Euterp.* ii. 10, 12.

aware that the nummulites embedded in those mountain ridges had once lived in the waters of the ocean. I am not you will understand endeavouring to show that there is no evidence that all the land of Egypt, with all its high hills as well as its plains, was once for a time under water. The testimony of Herodotus as regards the shells found upon the mountains is valuable, whatever we may think of his deduction, that "the valley below Memphis was once a bay of the sea." If masses of sea-shells had been discovered in the Nile mud, and in the sand which is mixed extensively with the mud, that might have gone far to prove the conjecture of Herodotus to be right; and it would be adverse to the usual supposition that these layers of sand have been blown over the mountainsides from the inland deserts. It would also have given some show of cogency to the argument of Professor Huxley, which at present it seems utterly to want. I say "some show of cogency" only, for here, though the evidence that no sea-shells are recorded as being found in the deposits, is very significant, their presence (at least to some slight extent) might be accounted for, as having been blown from the tops of the mountains into the valley along with the sands; and therefore would not quite establish that the valley had been once either an arm or "a bay of the sea."

Let us, however, proceed with Herodotus, and attend to some more of his actual facts, regarding this great valley of the Nile. After giving the whole length of the coast of Egypt as in his day 3,600 stades, he goes on:—

"From the coast, as far as Heliopolis, inland, Egypt is wide, being all flat, without water, and a swamp. But from Heliopolis upwards Egypt is narrow, for on one side there is the mountain of Arabia extending from north to south and south-west, stretching continuously upwards to the Red Sea; in which mountain are the quarries whence the stones were cut for the pyramids at Memphis, &c. And on that side of Egypt which borders upon Libya there extends another rocky mountain, covered with sand, on which the pyramids stand," &c.; and a little after he says, "Above this, Egypt again becomes wide."*

This passage would seem to be the ancient source whence Professor Huxley derived the idea that some of the pyramids are built upon "rock and sand." In Mr. Cary's English translation of Herodotus, published by Bohn, the words, "a rocky mountain and covered with sand, on which the pyramids stand," might for a moment just suggest this notion, which however a second moment's reflection ought to

* *Euterp.* ii. 6, 7, 8.

dissipate. The passage, read anyhow, ought to have been sufficient to put any one on his guard against transporting the pyramids from their real position on the *rocks* "on which they stand," into the valley of the Nile!

Here I must for a moment leave Herodotus, in order to allude to one other consideration affecting this important question, and which might of itself have been advanced as a sufficient argument against any assumed uniform rate of mud-deposit in the Nile valley. I refer to the great probability that the general level of the country of Egypt has been subjected to elevations and depressions, which of course would materially affect the rate of the Nile's deposits. It appears that Sir Gardner Wilkinson was led to infer that there has been a sinking of some parts of Egypt, judging from the present position of the tombs in the Delta called Cleopatra's Baths. These, he thinks, could not have been originally built so as to be exposed to the sea, which now fills them; but must have stood upon land once above the level of the Mediterranean. Sir Gardner adduces as additional signs of subsidence, some ruined towns now half under water on the Lake of Menzaleh, and channels of the ancient arms of the Nile itself, now submerged with their banks below the level of the water of that lagoon. Professor Huxley did not think it necessary to notice these facts adduced by Sir Gardner Wilkinson, nor the seemingly "cogent arguments" Sir Gardner founds upon them. No doubt it is much easier to settle complicated questions off-hand, in "professorial style," and "to snatch a verdict," especially when it may be done "with benefit of clergy!" But is this fair to one's audience, or to the public, or to Truth? Is that the way we are to teach our children "science," in the days to come, in our halls and universities?

But to revert to Herodotus. He tells us that in his day, that is, about five hundred years B.C., the Egyptians inclosed within embankments the areas upon which they had built their temples and monuments, and that these spots appeared to have sunk, and could be looked down upon from the surrounding grounds.

This is adduced by Mr. Brodie* as an argument in favour of a depression having taken place of the sites on which the temples stood, subsequent, of course, to their erection. No one will readily believe that the architects of Thebes or Memphis would have built city after city and temple after temple in positions where they would be annually flooded; and indeed there is a passage in Herodotus which shows that

* *The Antiquity, &c., of Man*, p. 56.

the cities were in his day still standing generally upon elevated foundations or on rocks in the valley. He says :—

“When the Nile inundates the country the cities alone are seen above its surface, very like the islands of the Ægean Sea; for all the rest of Egypt becomes a sea, and the cities are alone above the surface. When this happens, they navigate no longer by the channel of the river, but across the plain. To a person sailing from Naucratis to Memphis, the passage is by the pyramids; this, however, is not the usual course, but by the point of the Delta and the city of Cercasorus; and in sailing from the sea and Canopus to Naucratis across the plain, you will pass by the city of Anthylla and that called Archandropolis,”* &c.

Well, this being the case in the time of Herodotus, let us remember, that if we take Professor Huxley’s rate of deposit for the mud as a foot in a century, all these cities if standing at the present day would have been 23 feet nearer (if not below) the surface of the water than when Herodotus wrote;—or on the more moderate and “correct” calculation of 5 inches deep of deposit now in a century (and adding nothing to this depth for the narrowing of the valley), they would be some $9\frac{1}{2}$ feet less above water now, than twenty-three centuries ago.

Herodotus further mentions that—

“the priests had told him that in the reign of Mœris, when the river rose at least eight cubits, it irrigated all Egypt below Memphis; and yet [he adds] Mœris had not been 900 years dead when I received this information. But now, unless the river rises sixteen cubits or fifteen at least, it does not overflow the country. It appears to me, therefore, that if the soil continues to grow in height, in the same proportion, and to contribute in like manner towards its increase, those Egyptians below Lake Mœris, who inhabit other districts and that which is called Delta, must, by reason of the Nile not overflowing their land,” suffer for want of water.†

Leaving out his mere speculations and looking at his facts, they would seem to indicate that at this time the city of Memphis was not liable to be flooded as it is now; but only the whole country *below it* (or of a *lower level*) towards the sea. That of course is perfectly consistent with the lower ground much further up the valley and all round about, being more or less irrigated by the rising of the river.

So much then for the argument from the mud-deposits in the valley of the Nile.—And now for Professor Huxley’s next point—

* *Euterp.* ii. 97.

† *Euterp.* ii. 13.

THE ARGUMENT FROM THE NUMMULITIC ROCKS OF THE NILE-BASIN AND THE CHALK FORMATIONS.

I am glad that here we have no important question of fact to occupy our time. The character of the Nummulitic limestone strata may be admitted, as lucidly described by Professor Huxley, with a very slight qualification. He says the existence of the nummulites, and of other organizations of sea-habitants embedded in these strata, affords evidence that this nummulitic limestone was formed at the bottom of the sea. He also speaks of it as having been "deposited" there; and it is that word deposited which requires to be accepted cautiously, as we shall yet see. But he goes on,—

"Therefore before the Nile valley was formed, the land of Egypt [meaning this nummulitic formation] was down at the bottom of the sea; raised by subterranean forces; and must have existed not only 7,000 years, but all that epoch which by slow accumulation would have furnished such a mass of nummulitic rock, spreading as it does from Hampshire to China."

Then he asked, "How many years would this take? Thirty thousand?" And he replied, "More. The time which this process occupied was an enormous period. And even this is but as it were an incident in the history of this earth—no more than the shadow of a cloud passing over the history of the world." Then the Professor proceeded, (as described in the *Saturday Review*,) "to unroll the long series of geological formations which had preceded the chalk." Next he compared the old chalk formations to the chalk-ooze of the Atlantic now; and reminded his audience that chalk is one mass of the exuviae of foraminifera and other organisms that once lived and could only have existed at the bottom of the sea under the same conditions as they exist now. After which he said:—

"A million years could not have produced this chalk deposit of 1,100 feet thick,—whether less or more it makes no difference,—but it is clear that this world was *not* made 6,000 years ago."

I trust I have fairly epitomized Professor Huxley's statement. Now, I wish you to analyze it, and see clearly how much of it is certain, and how much is merely conjectural. In the first place we must take away the 7,000 years, he thought he had proved, for the previous mud deposit of the Nile; and therefore it is *not* certain that the nummulitic rock must have existed all that time. But then he says, we have all the long epoch required for "the slow accumulation" of the mass of

nummulitic rock. He omitted, however, to prove anything as to the rate of its formation. He assumed it to be a mere deposit, and that its accumulation was so slow as to take 30,000 years; but the whole of that is mere assertion and conjecture. He may be right or he may be wrong; but he advanced no argument whatever in support of *this* Scientific Doctrine. Well, how can we examine whether an argument is cogent or not, when we have no argument to examine? Might not that reply be now sufficient? Whenever he "brings forth his strong reasons," would it not be time enough then to consider them? When he dealt with the length of Egypt, with the Nile valley, with Scripture, with Herodotus, and with the time required for the mud-deposits, and gave us something tangible to examine, I think I did not shrink from the task. But what can I reply to this mere *ipse dixit* that more than 30,000 years were required for the formation of the nummulitic strata? Had he been nearly right on the simpler problems of geography and history he began with, and somewhat fuller in his statements of the facts bearing, for instance, upon the deepness of the scientific borings in the Nile valley, we might have been inclined to trust him more easily here. But, if he has been both reticent and wrong, and has signally failed, as I do think he has, to help us to discover anything like the probable time required for a mere surface deposit of mud, we cannot be predisposed now to accept his mere off-hand estimate for solving this deeper problem.

But do not think I am saying this in order to escape the necessity of saying more. I only wish to show, that I must now take another line in my reply, when there are no real facts to dispute, and no arguments of any kind to answer. At the same time I do not think it would be profitable to meet assertions merely with assertions; while still less could I presume to offer any mere assertions of mine against those of so distinguished a professor. I have indeed an advantage in knowing that it would be useless for me to attempt to palm off upon your understandings here, any mere vague and extravagant doctrines, without the least proof, and expect you to give them credit. Not being a "scientific authority," I can only expect your assent to what I may prove or disprove, or can show to be probably true.

Well, I think there is something to be advanced in reply to Prof. Huxley, which must lead you to reject the Geological Chronology which he chiefly relied on for discrediting the chronology of *Genesis*. He thinks nothing of 6,000 years. Even the 30,000 assigned to the nummulite formations alone, he considered as not worth regarding, when compared with the

enormous periods required for the other successions of strata all deposited and laid upon one another ; but especially as unworthy of notice when compared with the chalk. For his culminating assertion was, "a million years could not have produced this chalk deposit of 1,100 feet thick." He seemed prepared to rest his whole case upon this ; so here then let us now join issue. But I select the chalk not only as his strongest point, but also because, as regards the chalk, he favoured us with some show of argument, deduced from the analogy of what we know of the present chalky ooze of the Atlantic. I accept the analogy as a fair one, upon which a cogent argument might be based, bearing upon the old chalk formations. Let us now therefore examine how much of cogency may be discovered in the argument of Professor Huxley.

But here I regret to be obliged to point out, that he was exceedingly chary and vague in the information he thought proper to communicate, in order to establish the probability of his scientific doctrine. Probably all who heard him knew long before 21st November last, that chalk is mainly made up of microscopical shells, and that in drawing a chalk-line upon the black board, as he graphically did, the white mark was almost literally "a line of skeletons."

Perhaps, also, most of those who heard him knew long ago, all that he chose to tell them then, about the ooze of the Atlantic. Whether it was that he considered the argument from the ooze to the chalk as too obvious to require to be fully stated, or whether it was that its whole import was so clear in his own mind that he forgot to give it expression ; certain it is, that, except to say that the ooze is essentially a kind of grey chalk in the process of formation, and to call it a "deposit," he told us nothing. He told us nothing especially of the rate, either actual or conjectural, at which the ooze now accumulates in the Atlantic Ocean, though that was apparently intended to be the sole criterion for calculating the more than a million years for laying down the old chalk formations. Neither did he even hint to his audience *how* the Atlantic ooze is known or supposed to accumulate. Nor did he think it incumbent upon him to advance a single argument, whether cogent or not, to show that the old chalk formations must have been accumulated in precisely similar circumstances as the present ooze of the Atlantic,—except (about which there can be no question) that the one like the other accumulated at the bottom of the sea.

It was, therefore, in order to enable him to supply these omissions and to complete his own argument, that I ventured

to ask him at the time, to be good enough to explain, whether he thinks, or knows, that the foraminifera of the Atlantic ooze are merely deposited when dead, (for he had spoken of their "exuviae,") and by simply sinking down in that condition to the bottom of the ocean; or whether he thinks, or knows, that they are still alive at the bottom, and propagating their species there; in which case, I pointed out, the so-called "deposit" of ooze would not be a mere sedimentary deposit; for it would then chiefly *grow* by accretions to its surface at the bottom of the ocean, though it might also be increased from the sediment in the waters falling down from above. I do not know whether the idea had ever before occurred to him. Perhaps—as a new idea coming from one not within his circle of "scientific men," it may have struck him as not worth considering, or as merely absurd to suppose that the foraminifera are actually breeding now at the bottom of the Atlantic. And perhaps they do not breed there. But, if not, they must have been bred elsewhere. They are living organisms; and they are of that lowest class that generally increase and multiply with the most marvellous fecundity. And what I wanted to know was, what is the "scientific doctrine" respecting the Atlantic ooze, in order to discover, whether there was a true analogy and any cogency in the argument, in favour of the "scientific doctrine" that the old chalk formations were formed, or "deposited," identically as the Atlantic ooze is now. Professor Huxley, I am sorry to say, did not favour me with any reply to this inquiry. Perhaps, like some other professors I know, he does not like to be examined!

In the absence, then, of Professor Huxley's express teaching, I may say, that I am told that one scientific doctrine about the ooze is, that the gulf stream carries into the North Atlantic great quantities of the foraminifera, which are partly caught by or cling to jelly-fish, and partly sink to the bottom. Perhaps it is not really known whether when in the ooze they are still alive, and able to reproduce themselves, or not. But, if not there, I must repeat, they must have been bred somewhere else; and I think it must be admitted, that where they breed there they must accumulate with an infinitely greater rapidity than where they, or their exuviae, merely sink when dead to the bottom, after escaping the jelly-fish and such other inhabitants of the deep as may relish that kind of food. And again comes, of course, the question—towards the solution of which, however, Professor Huxley contributed nothing,—Is the old chalk merely also a deposit of dead foraminifera (if such be the character of the ooze of the

Atlantic); or, Is it a very different formation, that was accumulated by the fecundity and reproduction of living foraminifera that had never been washed away from their native beds in the bottom of the primeval seas where first they began to live? You will observe the two cases, as now supposed, are no longer analogous. If this supposition be wrong, and a true analogy can be established, it must be obvious that this will very materially affect the cogency of the argument in support of Professor Huxley's doctrine. The importance of having some actual knowledge to guide us by analogy, some real "science" of the formation of the Atlantic ooze, cannot be over-estimated. For there is still a further analogy, which Professor Huxley pointed out, between the chalk and nummulitic strata. Both have been evidently formed in the beds of the ancient oceans; for both are full of the dead remains of sea-inhabiting living organisms. It will make all the difference to our argument and analogy, as regards all such marine formations, if they grew up at the bottom of the seas, like coral reefs now, by the reproduction of their living foraminifera and nummulites, &c., *in situ*; and if these were *not*, after having grown and been reproduced and multiplied elsewhere—for I apprehend I may assume that foraminifera are not eternal atoms!—washed away from their beds, and carried hither and thither by some ancient gulf stream, to feed whales and jelly-fish, while only a remnant of them could escape to fall to the bottom as a sediment or deposit of ooze.

In asking Professor Huxley for merely a statement of the scientific doctrines as to these essential points, I ventured to hint at another analogy as regards the now admitted growth of peat, which—as "a word to the wise"—might have enabled him to understand the importance of my inquiry. At one time, and not very long ago, it was scarcely known as a scientific doctrine that peat really grew at all, and even now its rate of growth is kept well under check. One eminent man of science, (who for years was himself kept down by other men of science, though lately he has become almost "the rage,")—I mean M. Boucher de Perthes,—has taught that the growth of peat could only be computed at the rate of about the fifth of an inch in a century; whereas Sir Charles Lyell in his *Principles of Geology* alludes to the growth of a peat-moss in Lochbroom in Ross-shire, to such an extent of thickness, in "less than half a century," as to be fit to be dug for fuel by the inhabitants. He also mentions, in the same celebrated scientific work, that the Roman roads in Scotland are now in some instances covered over with peat-moss,

no less than eight feet in thickness.* According to M. de Perthes' doctrine these Roman roads must have been formed 48,000 years ago! So much for the unity and certainty of some scientific doctrines, as to the time required for nature's operations, even when "facts" are within our reach.

And here let me ask, Suppose Professor Huxley to agree with the eminent Frenchman, what might be thought were I to put to him the puerile and uncomplimentary question, whether he is prepared to argue that the Roman roads were made and "put under" the peat after it had grown, rather than honestly admit the fact that the peat has grown after the roads were formed?—He ought not, in my opinion, to have introduced that kind of interrogation, with reference to the Nile mud and the pyramids; and I make this allusion by way of warning that however eminent may be his position, it would really be for his own credit, and perhaps safer, to avoid that style of controversy. Even if our arguments fail, we may at least avoid mere gratuitous and unprovoked sneering.

To revert to the chalk and other sea-bottom formations. I believe that truly scientific men do not profess to know the probable rate of their growth. A calculation has however been made that taking one single shell of the foraminifera, only one ten-thousandth part of a cubic inch in size, and granting that from one such organism 10 only would be produced in the course of a whole year, and that the original progenitor would then die; and supposing each one of the 10 merely to multiply at the same exceedingly moderate rate, and to produce 10 each per annum;—and so of the 100, and of the 1,000,—10,000, and 100,000 afterwards produced;—the result would be that in less than a single century,—in less than 100 years of such slow reproduction and growth,—a solid mass of the exuviae of the chalk foraminifera would be produced more than equal to the cubic contents of the whole earth.

I know that for a moment this will appear incredible. I need only ask, Is it true? It is no mere vague conjecture. It is a matter of figures and computation and of absolute demonstration. It is not a mere vague assertion, of 30,000 or of a million years, without the least data to prove it. If it be said that the foraminifera or the nummulites cannot reproduce ten each of their species per annum, let "science" tell us that.

* Vide *The Age of Man, &c.* By Professor Kirk, Mem. Vict. Inst., pp. 75, 76. (Lond., Walford, Jackson, & Hodder.) I may add, on the authority of Professor Kirk, that certain moss-farmers say that the peat on their farms grows at the rate of $2\frac{1}{2}$ inches in a year.

If the scientific do not know what is the fact, let them give any reason for thinking this rate improbable. If they say five only each in a year, we shall recast our figures; and even then we shall find, that we want neither a million, nor 30,000 years, not even more than a single century or two, to account for all the chalk and all the ocean ooze there is now in the world. If they will allow the foraminifera to breed at all, and at the rate of any of the other lower organisms of which they have the most perfect knowledge, and if they will grant us but one to begin with, we shall be able to refute these mere fanciful "scientific doctrines" that are totally unsupported by proofs or cogent arguments.

But those who cannot believe that even a single individual of the foraminifera could have come into being of itself, and who consequently believe in Creation, do not of course suppose that when the waters were commanded to "bring forth the living creatures after their kind," that only one or only a single pair of the foraminifera were then created. Consequently any calculation as to their subsequent reproduction that is based upon there having originally been only one, is a mere concession to the adversary, and no part of our own case. Most likely millions of such creatures would start into life at the first fiat of the Great Creator. And though probably the rate of their propagation is very much greater than was supposed for the sake of argument, they could not continue thus to go on increasing, from the want of food, or for want of carbonate of lime or the other material required for the formation of their shells. The watery "soil," if I may use the phrase, would after a time become exhausted here and there, while millions of them would be sucked up by jelly-fish or otherwise disappear, in the notorious "struggle for existence," which we may admit to be powerful to slay and destroy, though not to give life in this world. But, if we compliantly *suspend Theology*, and, as is now the fashion, leave out Creation altogether—although our reason cannot find any other probable beginning of things;—and if we merely commence with the "one only" of these atom-like foraminifera, got anyhow, we have seen how rapidly the chalk formations may have grown, and in that way become "deposited," at the bottom of the ancient seas. There the chalk no doubt once lay, and there—have we any reason to doubt?—the minute foraminifera, that built it up, once lived and increased and multiplied. Are we not now entitled to ask for some equally definite data and equally cogent argument from the other side, before we are expected to come to some contrary conclusion, and to believe in these indefinite thousands and millions of years?

And now it might well be supposed I have said enough, and that it is time to put the question, Have I answered Professor Huxley, or not? Well, I think I may claim to have shown that his 1,100 feet of chalk may have taken much less time to "deposit" than even the mud of the Nile! His million of years for the chalk may have been less than half a single century; and there is not any reason to suppose that when the nummulites lived in the ocean they were less prolific than the foraminifera. But he had one other argument still to complete his sorites. His arguments in detail may have broken down. But there were the arguments when all put together, and from all the strata heaped up and cumulating upon one another.—Let us now then look at this; namely—

THE ARGUMENT FROM THE SUPERIMPOSED STRATA AND THEIR FOSSIL REMAINS.

There was first the time required for the deposit of the mud. Before that, there was the time required for the formation of the nummulitic limestone; and before that, the time for each of the long series of geological formations which preceded the chalk; then the more than a million years required for the chalk alone. And even if we find, that we may reduce the period for the chalk to half a century, and so the time for each of the other formations in detail, with greater ease than the time required to lay down the superincumbent mud; still we are also required to observe, how these strata all come in succession, after and upon one another, and now we must count up the times required for all that. Not only so, but the learned Professor wound up his discourse in the following words, enunciating what must have been generally regarded as the most startling of the scientific doctrines which he put forth in Sion College,—I mean startling merely because enunciated by Professor Huxley,—for even it was "nothing really new":—

"There is positive proof (he said) of three successions, of three revivals of the living inhabitants of this world. Do we not see then the unknown previous duration of this earth?"

Afterwards he concluded his discourse as follows:—

"These views, of which I as the Minister of Science am the exponent to-night, are held by men who are as Christian in motive and practice as you. These doctrines are held by men who think deeply and who have children to come after them whom they desire to instruct wisely. They are held by the best of men; they are held out of no wantonness or irreverence or eccentricity. They are held by men who seek to discover to themselves and

to present to others Scientific Truth. I ask you to remember this, to consider this ; and *then* I ask you to judge us."

I hope I may be pardoned for having read this last quotation, which goes beyond the point under consideration. The fact is, I could not refrain from giving you the pleasure of hearing—even if it may be of hearing over again—this eloquent peroration, this admirable appeal to the highest feelings of our nature as men. But having done so, I would respectfully say, let us on the other side be judged as considerately and fairly. I will further say this, I do not know a person who would dare to reject a single scientific doctrine which he really believed to be true. I do not even understand how it would be possible for a man to do so. Men may shut their eyes, I know well, to proofs or arguments on either side. On both sides they may often take their science or their theology, perhaps contentedly, at second-hand. But those who enter the lists to discuss those matters have nothing to do with such.

I trust it has been thus far seen that I do not shrink from looking all the facts and issues fully and fairly in the face. Were it not that it was next to impossible to go over the whole system of nature in a single lecture, we might even complain that Professor Huxley went no lower than the chalk deposits,—the mere commencement of the Cretaceous system, or the surface of the Secondary Formations. For we must also remember the fact that, below the Cretaceous beds (that is, if the usual order of formations has not recently been "turned upside down"!) we come to the Wealden, the Oölitic, the Triassic, the Permian, Carboniferous, Devonian, and Silurian Systems; all these having each their numerous subdivisions; and, after these, we have still to go deeper and deeper, till we come to the Crystalline rocks, and the "fundamental Granite," belonging to what was once called (if I may now mention them) "the Azoic ages"! Well, then, how are we to deal with this great world, if, beginning with its surface, we proceed to strip it successively in imagination of all its various strata, one after another, as we stripped the Nile valley of its paltry annual deposits of mud; and if afterwards we essay to get rid of the non-sedimentary conglomerates and other masses that lie below? I know that, as regards all living organisms of the earth, Professor Huxley, in his *Man's Place in Nature*, has announced *his* readiness to begin them all with an atom-like "egg"! But, then, surely he does not believe that the marvellous, hidden life within such eggs could produce the least visible growth of the organisms unless there were pre-existing materials which it could appro-

priate and convert to its use? There was, I am quite aware, an ancient theory that began the world or the universe itself, with an egg, and made it thus to grow from almost nothing, I don't know how, to its present dimensions. That was a *thorough* Darwinian system! But, then, it was invented before the modern scientific doctrine, that matter can neither be lost nor destroyed, was put forth as scientific truth. Now, as regards this doctrine, that matter in our experience is never increased, nor decreased, nor destroyed, as it goes through its varying phases or Protean changes, I am glad to be able to say,—“heretic” as I am accounted, and truly am, as regards some of the most important scientific doctrines,—that I consider this particular doctrine as nearly absolute scientific truth as anything ever propounded in philosophy.

Well then, accepting this doctrine, let us now strip the world of its mud, and of its strata, and its crystalline rocks, down as deep and as far as we please—for this we may do *in our imaginations*!—and what can we make after all, even in imagination, of the matter we thus strip off and try to get rid of? Was it nowhere,—was it not in existence,—before it was laid down as now, in its beds of strata or in the rocks underneath? Let Professor Huxley tell us that! If his answer is,—(and it is the *only* answer he can possibly give, if he will not tell us that new matter can grow and comes into being day after day.)—that all of it must have existed in the world, in one form or another, before it was arranged under present conditions,—then, that is just *our* argument who believe in One Creation of matter, or “of all things visible”! Men may imagine as they please, what has been laid down here or there, at this time or that, but all material things so arranged must still have before existed. I find, however, that I am diverging into considerations quite beyond the limited range of Professor Huxley's lecture; to which I must therefore return.

And now as to the last of the scientific doctrines of which the learned Professor called himself the exponent. No doubt you are well aware of the doctrine of special creations deduced from the apparent succession of life upon the globe. And this doctrine the Professor's words do seem to teach. But perhaps you had reason to think this was a doctrine that had been given up, or (as it has been euphemistically described) one “which was slowly yielding to other views.” It was at any rate something new, to understand that it, or anything like it, was held by Professor Huxley! However, if he has adopted it (as he has some other new “scientific doctrines,” within not many years), that may by some persons be regarded as a testimony to its probability. But if I remember aright, and

have correctly quoted his words, I must observe that the extent of the Professor's conversion is extremely slight. There were once no less than twenty-nine supposed successions of life on this earth. Six special creations at least were long in favour, of course with enormous intervals between. Professor Huxley only speaks, however, of "three successions—three revivals;" and it is fair to observe that *he* does not say "creations." We, however, have not so much to do at present with his full opinions as to this; but only with what he chose to enunciate as proved by science, and what he advanced as deducible therefrom.

His argument was that there were proofs of *three* successions or revivals of life in this globe, because of the differences found in the fossil organic remains in the strata superimposed upon one another. But I think you will admit that this is a subject far too large to be entered upon minutely at the end of this already only too long discourse. Yet still I must endeavour to convince you, that at the present time it would be most unwise to allow our children to be taught that even "three revivals" is really "scientific doctrine." But as "time hastens on,"—and I, unfortunately, have not unlimited periods of time at my disposal,—I must, in despair, at last have recourse to "scientific authority."

Well, one President of the Geological Society of London, Mr. Hamilton, thus expressed himself in his annual address in 1865 :—

"We are daily becoming more convinced that no real natural breaks exist between the Faunas and Floras of what we are accustomed to call geological periods." *

So he does not agree with Professor Huxley !

Another President of the Geological Society, in his anniversary address in 1862, called in question the contemporaneity, or identity of date, of what are called the *same* strata, in different parts of the globe; and he went so far as to urge also this :—

"Those seemingly sudden appearances of new genera and species, which we ascribe to new creation, may be the simple results of migration."

But the President of the Geological Society, who thus expressed himself in 1862, was the same Professor Huxley who taught the doctrine of "three successions—three revivals," to the clergy at Sion College last month ! Are then the doctrines of migration and revivals reconcilable ? It is not for me now

* Vide *Journ. of Trans. of Vict. Inst.*, vol. i. p. 38.

to prove that they are utterly repugnant. If I only succeed in convincing you, that neither the one nor the other ought to be absolutely accepted as "scientific truth," at least without further inquiry, I shall have done enough. For it is only our duty, you may remember, "*simply* to hear and believe,"—"when all the professors in the world announce a certain order of Geological succession!"

It is the fashion now, as we very well know, among a certain class of scientific men, to deny that some great convulsions of nature or cataclysms may have changed the face of the earth,—as by throwing down mud and other materials, perhaps like the masses of whole continents at a time,—or by rending the earth asunder and swallowing up tracts of country, not merely like that now forming the great sea-channel between the chalk cliffs of England and France, but even spaces of world-wide magnitude, as between Europe and America,—and thus leaving, like upheaved mountains, sometimes tilted rock-ridges, as of the nummulitic strata that form the basin of the Nile, or the steep and perpendicular cliffs of the old red sandstone, now lashed by the angry waves of the Atlantic, and the roll of the North Sea waters, at Cape Wrath and on the coast of Caithness.

But if, on the other hand, the mountains of the world be, as they are by some scientific men regarded, literally "upheavals" that have been erupted by the force of subterranean or volcanic fires, then the convulsive force required for this must be regarded as still infinitely greater; and the fearful chasms and terrific cataclysms that would be consequent upon this tearing of the earth's crust asunder, when heaved into larger space and stretched upwards and outwards, we may easily perceive, upon reflection, must be inconceivably greater than upon the more probable supposition of an occasional falling in of the earth's crust and filling up and consolidating its interior. The waters alone which spring among the hills of ten thousands of rivers that pour their floods into the seas, must operate with the mighty force of an infinitely powerful hydraulic engine, which day by day, and ever, is pumping and working, and gradually undermining the earth, and changing the local intensity of the pressure of that most powerful of material agencies, the constant force of terrestrial gravitation.

But if the idea that many of those apparently successive generations were possibly contemporaneous and embedded in different places about the same time, and that the strata containing them may have afterwards been transported somehow, during some ancient convulsion of nature, and laid upon one

another,—if this, I say, appears to any too startling a conception; let me quote briefly the words of some other eminent geologists, as to the startling changes that are *known* to have taken place in the strata of this earth. In Professor Ramsay's address to the Geological Section of the British Association at Nottingham, in 1866, he says :—

“The Silurian strata in North Wales are now to a great extent intermixed with igneous rock. . . . All the rocky masses of which the region consists, both igneous and aqueous, have been disturbed and thrown into sweeping undulations formed of curved strata thousands of feet thick, by those agencies, whatever they may have been, that at a later date produced disturbance.”

He goes on to say, that even those who have witnessed these contortions, can have no conception how still more marvellously the strata have been disturbed elsewhere, as in the Alps :—

“There (he says) we find areas as large as half an English county, in which a whole series of formations has been turned upside down.”*

And what is now the scientific doctrine respecting the so-called igneous rocks mentioned in the above quotations? At one time, you may remember, it was taught as “scientific truth” that granite had an igneous origin; and it was upon “the fundamental granite” that the sedimentary strata used to be laid down. Can any geological “exponent” now tell us, upon what the sediments of the seas are even conjectured to have been deposited? I am not aware that even speculative geology has yet invented a bottom for the waters of the globe, since the fundamental granite failed them. For what is this granite now found to be? In a paper read by Mr. Geikie in the Geological Society, and in a paper in the *Geological Magazine* for 1866, he says, that the sand-stones and clay, as well as limestone in Ayrshire, can be seen passing into trap and granite; and he adds :—

“At last I am therefore forced to conclude that the crystalline rocks described above have resulted from the alteration, *in situ*, of certain bedded deposits.”

In like manner writes Mr. Hamilton in his annual address, already quoted :—

“It was formerly supposed that the crystalline rocks, particularly the granite, owed their origin to igneous action. Now it is well known that these

* *Report of Brit. Ass.*, 1866, pp. 46, 47; and *Journ. of Trans. Vict. Inst.* vol. i. p. 370.

granites are chiefly arranged in layers. The granite passes into gneiss, and gneiss into mica-schist and talc-schist; and this is again closely connected with the green and grey slates; and it is well known that many of these rocks formerly considered as plutonic [*i. e.*, by the scientific doctrinaires] are really metamorphosed rocks.”*

Sir William Logan also confesses, when speaking of the Laurentian limestones:—

“We do not yet know with certainty either the base or the summit of the series.”†

In the *Geological Magazine* for January, 1865, we also find the following:—

“Judging from analogy, then, the Eozoön rock of Canada was the foraminiferous formation in one part of an ocean which elsewhere may have borne manifold and higher species, and buried them in sands and muds, that have since lost all form and feature by the metamorphosis of age and pressure, or which were altogether shorn away by wave and weather when the old ocean-bed was lifted up.”

I might quote more, but your patience must be wellnigh exhausted. I have made these quotations chiefly from our own *Journal of Transactions*, expressly to show that in this Institute we have here an antidote to such mere *quasi* “doctrines of science” as have been preached at Sion College. We have all moralized with Shakspeare as to the transformation of “the dust of Alexander” into loam that may have been used to “stop a beer-barrel.” Geology now forces us to reflect, that the very granite of “the everlasting hills” may have originally been built up by foraminifera in the lowest depths of the seas! But that need not disturb the faith of the Christian clergy or of any other believer in the old Sacred Scriptures. On the contrary, it rather suggests to me a text, if you will allow me now to take a text, at the end of my discourse, on the promise that I shall attempt no “exegesis.” It has come to my mind more than once, as I have followed Professor Huxley from the nummulitic limestone and other strata down to the chalk, and at last to “the fundamental granite.” It is this:—

“THE EARTH IS THE LORD’S, AND ALL THAT THEREIN IS; THE COMPASS OF THE WORLD, AND ALL THAT DWELL THEREIN:—*FOR HE HATH FOUNDED IT UPON THE SEAS, AND PREPARED IT UPON THE FLOODS.*” ‡

* *Journal of Trans. Vict. Inst.*, vol. i. p. 32.

† *Ib.*, p. 357.

‡ Psalm xxiv. 1, 2.

CONCLUSION : THE PRACTICAL LESSON.

In conclusion, I must crave your indulgence to be allowed still some little time, in order to answer Professor Huxley quite completely as to the issues of this great question, as he was pleased to put them before the clergy in Sion College. I have done the learned Professor the justice to say, that nothing could exceed the earnestness of his tone ; and I am sure that he did not in the least exaggerate the importance of the fact, that there are two adverse schools of thought, which exist among us, and which do rather tend to diverge more and more from one another. Being a distinguished leader of opinion in one of those schools, I think he undertook a solemn duty, in endeavouring to explain to the clergy the nature of the arguments from which he has arrived at his convictions. It was, however, absurd to suppose that such a mighty question could have been put upon a satisfactory footing in a single unrecorded discussion. The only fair and almost rational course, I ventured to point out ; but Professor Huxley said he thought it would be inconsistent with his dignity to appear before what he called "the tribunal" of the Victoria Institute. In inviting him to come here, no idea of any tribunal ever entered my mind, except that of the reading and intelligent public ; and were these polemical discussions at Sion College to be reported fully and printed, they might of course be as useful there as in any other place.

But I venture further to say, that Professor Huxley made another and a serious mistake, which still more lessened the usefulness of his address, in so utterly underrating the mental capacity and knowledge, and seemingly the honesty, of those whom he addressed. Consequently, while he did but scant justice to his own side of the question, he utterly misapprehended, and so completely misrepresented, the other. The tone of his whole address became therefore (though as was very evident unintentionally) offensive. It seemed as if he thought, that only himself and those who think with him were honest and well instructed as to a few quite notorious geological facts ; and that the clergy were very ignorant, and not quite candid, nor willing to admit the force of evidence, or to give credit to scientific men for the honesty of their convictions. While he wasted great part of his time in raising imaginary difficulties which nobody felt, and in demonstrating simple points which no one would dispute, he said little or nothing to justify the larger issues involved, or to prove the necessity

for his constant demands for the illimitable periods of time which constitute the whole of his geological chronology.

I have already called attention to his utter silence as to the well-known differences there are, and ever have been, as regards the chronology of Genesis. I must also notice his tone throughout, as if there were absolute certainty in every professedly scientific conclusion he chose to urge against the Bible. The omission to state fully his opponents' case, was as nothing to the still more one-sided manner in which he advocated the views of the mere *party* whom he truly represents.

Those ominous warnings to the clergy, to remember that the Bible chronology must yield to the certainty of scientific opposition, followed by his but ill-sustained appeal to the Nile-mud and some of the sedimentary rocks, are not without a parallel, which as a scientific man he ought himself to have kept in mind, and perhaps, with the perfect candour he professed, to have brought to the remembrance of his audience. Surely Professor Huxley has not already forgotten the same kind of ominous warnings, in Dr. Temple's and Mr. Goodwin's contributions to the *Essays and Reviews*. True the Mosaic cosmogony of "the Hebrew Descartes" was not then said to be in danger from mud and chalk, or the latest scientific convictions of Professor Huxley. But the danger was declared to be quite as imminent; the warning, quite as peremptory, was boldly put in print; and it was the hot-fused granite of Laplace that was then to pour destruction upon the earth and waters as created in *Genesis*! And how has the old-fashioned world passed through that fiery ordeal, and withstood "the jostling" with which it was threatened "from sturdy growths of thought"? Most bravely, as you know! Where is now the "scientific doctrine" of the *Essays and Reviews*?—the doctrine that regarded this earth as "once fluid with intense heat, spinning on its own axis and revolving round the sun"? Was ever any doctrine regarded as more absolutely "certain"? Some of the well-meaning clergy actually believed it to be scientific truth! For in the *Replies to Essays and Reviews*, one writer, who is both clergyman and astronomer, considered it even "important to observe that the earth was once in a fluid state!" And yet, in 1864, Sir Charles Lyell, as President of the British Association at Bath, described this important doctrine as merely a "theory" that was "altogether delusive!" And so, too, it will probably be with Mr. Huxley's mud and chalk theories, and the millions of years he demands of our faith, for his uncreated, bottomless deposits. As yet he has not even attempted any proof so imposing as that which Laplace put forward, as mathematical

demonstration, for the nebular hypothesis. The more definitely he states his views, however, the more rigidly he works out his principles, and the plainer he announces his conclusions, the better will all who differ from him, I am very sure, be pleased. Let him tell us how he begins his world, now that the fiery granite-mist has turned out to be a "delusive" foundation. Let him step in where Laplace and Mr. C. H. Goodwin did not fear to tread, and give us something rational to fall back upon, before we quite give up the time-honoured Mosaic cosmogony. If he can't, let him say he can't; and, meanwhile, let "the clergy" wait. And if Professor Huxley will twit them with their Thirty-nine Articles; let them ask him to produce a Fortieth, being a coherent "scientific doctrine," that even attempts to explain the existence of the world; and which he can truly say has been held by six men of science, taking these at his own estimation, or even by himself, for no longer time than merely the last six years! Let him do this in the noble spirit of a man who would "rather die than lie;" and let him keep nothing back either of his past or present beliefs, that the clergy and the public may know with what constancy and cogency of arguments he has taught and still teaches "scientific doctrine." The clergy and the Christian laity have long had their duty inculcated in the manly sentence of one who knew what it was to suffer and to die for truth's sake:—*Prove all things; hold fast that which is good.* Be assured, that this comprehends the range of "all things" which we call nature. Did not the grand old Hebrew prophets, long before, denounce the vain teachers of their day, *who regarded not the works of the Lord, neither the operations of His hands?* There is another old sentence to encourage us, *Magna est veritas et prevalebit.* By the clergy especially, permit me to say (since they have had lay-advice elsewhere), this ought to be well remembered; for to them has been especially committed the teaching of that Truth, which, here, we still hope "may flourish forth in the earth." They should take heed what they put in its place, or venture "to preach from their pulpits." They should especially "take heed to the doctrine" they teach, when they have it in their power to know, that again, and again, and again, what has passed for a time among men as "the wisdom of this world" has been afterwards proved to be *foolishness*.

On the motion of the CHAIRMAN, a cordial vote of thanks was passed to Mr. Reddie for his paper, and to the Rev. Dr. Thornton, who had read it in the absence of the author.

Rev. Dr. IRONS.—I rise, although the hour is somewhat late, because I was present at the meeting to which the paper refers, when Professor Huxley addressed the clergy at Sion College. I do not know whether that gentleman is here to-night: if he be, I should prefer at once to resume my seat, and to hear what he has to say in reply to what I believe to be the unanswerable paper of our Honorary Secretary. (Hear, hear.) If he be not here, however, I do hope that he will be duly informed by scientific and other friends who may be present, that we shall be happy to see him at our next meeting, that we will give him the most cordial reception, and that he shall here be allowed to state, as definitely as he can, what are those positions of a scientific kind which he imagines the clergy to repudiate, and which he asserts that we regard as entirely contrary to Holy Writ. I, for one, am not aware of any such fixed scientific truths which I and my clerical brethren, who have carefully considered these subjects, repudiate. (Hear, hear.) And I rise for another reason; and that is, to protest against the kind of issue which has been raised by Professor Huxley, and admirably met, I think, by Mr. Reddie's paper. It is an unfair thing for a man to stand up in the midst of his brethren and to say that he will not there declare his own opinions on a particular subject, but will only say to a certain extent that which he thinks he may venture upon, leaving his hearers to guess the rest. That was the position which Professor Huxley assumed at Sion College. With respect to the particular questions which he raised, there was no time then, any more than there is now, after an address which lasted about the same time as the paper which has just been read, to enter into a detailed discussion; but the injustice which had been committed was so deeply felt by me at the time, that I was obliged to ask Professor Huxley whether he meant to say that the clergy were fools or knaves? Whether we were so idiotic that we could not comprehend the arguments to be deduced from scientific facts, or so thoroughly dishonest that, comprehending them, we would not own the truth? He said he meant to make neither of those charges. I accepted his statement, and thanked him for the disclaimer, but I asked him further, what it was that he did mean? (Hear, hear.) If he were here, he might tell us now what he did not tell us then. You will recollect, Sir, for you were there then, as well as on another occasion, when Mr. Huxley was with us, that an answer was given to him which I think he had not expected. We showed him, I mean, pretty clearly that there is no truth which has been put before the mind of the thoughtful Christian philosopher in any age which he has ever been wont to shrink from. (Hear, hear.) But we are probably most of us acquainted with the statement of Sir W. Hamilton, that there is a certain class of scientific persons who, being engaged in a very limited circle of studies, hold exclusively to a few ideas, and almost lose their logical faculty. I could not help being reminded of that when I heard Professor Huxley's address, because he entirely confounded two things which the logical mind would have distinguished from each other and kept entirely apart. He confounded hypotheses and facts. (Hear, hear.) If there be anything which a clear-headed scientific man ought to be qualified

to distinguish in science, it is between that which is theory and that which is fact. One who was present at Sion College went so far as to demand—and I say it with regret, because I have a respect for him—that we should regard a scientific dictum as an oracle—the word “oracle” was used. I asked immediately, how were we to accept the oracle when it spoke in different senses? (Hear, hear.) We have had in our own lifetime a geological oracle giving us as absolute truth first a fiery theory, then a watery theory, and lastly, a very cloudy theory. (Laughter.) Which is the true one? (Hear, hear.) At present even Sir Charles Lyell himself is in doubt about it. What, then, are the unfortunate clergy to do under such circumstances? We read the books of scientific men, although they do not read ours, and we know something of both sides of the question, while they are ignorant of theology. We cannot understand what it is in this matter of geology which they wish us now to believe. As I said just now, they did not seem to comprehend the difference between hypothesis and fact. A hypothesis may be naturally and honestly held by any man. You have your opinion, I have mine, and another man has his, all of them different from each other. We all of us have a right to our own opinion; but if we choose to hold an opinion contrary to the facts, we must take the consequences. No man can ultimately escape, if he really will not accept the facts of the world. It is ridiculous, then, for scientific men to come forward and tell the clergy to accept, as the facts of science, what are really only the theories and hypotheses of scientific men. (Hear, hear.) They know very well that no man in his senses can deny a fact. The denial of a fact can be of but brief duration, but the denial of a mere hypothesis is the right of every intelligent being, if he chooses to exercise it. But not only do our geological friends hold certain hypotheses. Let us look at our chemical friends—another branch of scientific men. They told us, when we were boys, that the atomic theory—one very similar to that of old Epicurus—propounded with great authority by Mr. Dalton, a Quaker, was a chemical truth. The University of Oxford, that great obstacle of learning, as Mr. Huxley would conceive it to be, was so eager to meet even a Quaker with a scientific truth in his hand, that it summoned this Quaker, Mr. Dalton, to the University, and conferred on him—I was present at the time—the honorary degree of D.C.L. for his discovery. But at the last meeting of a great scientific society—the British Association—held in Dundee, in this very year, the president told us that the atomic theory is a mistake. Now, what are we to think of these scientific men? I call upon them not to blow hot and cold—not to say that we are to believe one thing in 1865, and another thing in 1867, on the same subject. When we protest that, after weighing their theories calmly, and giving them all our attention, we cannot accept them, they get very angry because we do not fall down and worship them as oracles! I think it is quite time that this tone should be entirely scouted. (Hear, hear.) It is time for scientific men to understand this 19th century in which they live. We are thinkers as well as they, and I would say to

them all, there are no books they publish which we do not carefully read, and very few truths in their geological studies which we may not remember to have found thirty years ago in Humboldt. We all welcome a theory when it comes to us, and give it the best attention and consideration in our power; but scientific men should not be angry with us for not at once accepting theory for fact. We repudiate, too, this trifling on the part of quasi-scientific men, who meet in hot crowds at Nottingham, Oxford, or Dundee, in order that they may be thought very learned or very clever. I say, we repudiate the notion that these men are to be our teachers because they choose to call themselves philosophers; but immediately we make the repudiation, then all these gentlemen are down upon us with Galileo. I know at once, when a scientific man gets up, that this is sure to come out; but there is something simply ridiculous in it. I am not aware that science has much to boast of in its martyrs, and that subject, I would tell them, is a question not of science, but of martyrdom. Whenever there is a shadow of a martyr in the distance for our scientific friends, they give a shout of exultation. They have got a case—a real case, and they bring it out with delight. They give a sort of feminine scream at the very thought of marshalling a scientific martyr against us. (Laughter.) But we have martyrs in theology as well. (Hear, hear.) If Professor Huxley had been present, I should have said a little more in pointing out what is unworthy of scientific men. We who are trained in the school of Christ, our Master and Lord, have a love of truth, because we have a love of Him. We know that what He has said will hold true; and when the scientific man tells me that his theories are sure to turn out right, and that the theologian must be convinced in the long run, I tell him that the very heaven he points to for astronomical truth, the very earth he digs for geological truth, will all pass away, but there is something greater which will not. "Heaven and earth will pass away, but my word," says our Master and Lord, "will not pass away." (Cheers.)

Rev. JOHN MANNERS.—I should like to add one or two words to what has just been said. I was not present at Sion College when Professor Huxley delivered his address, but I should much have liked to have been there, in order that I might, in a conversational tone, have asked a few simple questions, which I am sure we should all have been glad to have had answered in a straightforward and satisfactory manner. We are all actuated by the one object of desiring to ascertain the truth of these matters, and I am convinced we shall find that all scientific truth revolves round Christ as a living Centre. Just as all things had their origin from the eternal Word in the beginning, so we shall find that all true living science has its origin in Him, and is sustained by Him, who is the truth, the light, and the life of the universe. Without verging into theories of Pantheism or anything of that sort, we shall find this absolutely true, and if it were not now too late in the evening, we might throw out a few suggestions to show how all truth radiates round the One Centre, just as the sun's rays luminate from and radiate around the sun itself. Just one word about Cambridge and the ignorant clergy. It turns out, according to Professor Huxley and others—for

there are many of his way of thinking on the subject, and who regard us as either knaves or fools—some of the greatest astronomers and doctors in the highest branches of metaphysics at Cambridge have been clergymen. Take such instances as Challis and Earnshaw, who solved the difficult question of the differential calculus, and turned out some things that men of science have never been able to evolve from their manipulations. Take again such men as Whewell, Sedgwick, and Peacock, who were all clergymen; and a number of other men in orders, who never let a truth pass without giving it a most careful and searching examination. Take, again, our Chairman to-night—one of the highest authorities we have on the subject of crystallography. (Hear, hear.) I firmly and heartily believe that all the true principles of science are in accordance with the Bible, and are to be found to some extent there stated, though not in algebraic or analytical form, nor according to the forms of Euclid. And that has been necessarily so, because the scientific truths touched upon there, it was not necessary to state in detail. In the first chapter of Genesis there is the passage, “And darkness was upon the face of the deep.” I should just like to ask Professor Huxley what is darkness, and what is real, true light; and if recent experiments with the spectroscope are reliable, it will be found that the Biblical account is in harmony with scientific investigation. I know that scarcely a scientific book of any character at all ever comes out without its falling under the close scrutiny and attention of the clergy, and I know they find that true science, and indeed everything else which tends to the healthy development of the mind, are all in perfect harmony with the living truth. (Cheers.)

Rev. DAVID GREIG.—I should like to make one observation which I think ought to be borne in mind as very important in discussing this so-called difference between science and religion. Geology is generally termed a science, but I would say that it is not, and never can be a science properly. (Hear.) It can never be more than the merest conjecture. It differs totally and essentially from mathematical science, from mechanical science, and from chemistry. It can never be more than conjectural, because you can only reason with certainty from cause to effect, and when you draw inferences from effect to cause, as in geology, you can only conjecture. (Hear, hear.) We only know the effect in geology—we have a succession of strata, and we can only conjecture as to the cause which gave them their peculiar formation and position. If you have historical testimony opposed to your conjecture, whatever be the value of that historical testimony, the conjecture must inevitably give way to it. I am not a geologist myself, and I have a very indistinct recollection of Sir Charles Lyell’s description of the various strata; but I say that if you dig down three of those strata, said to have existed for countless ages, and find a trace of the old Greeks or Romans in the stratum below them, that historical fact would be sufficient of itself to throw overboard all the theories as to the immense ages during which the three upper strata were supposed to have existed. (Hear, hear.)—

The CHAIRMAN.—I may say that Herculaneum is a case exactly in point; that was the result of some of the excavations at Herculaneum.

REV. DAVID GREIG.—Suppose the Bible tells us as an historical fact that the world was formed some 9,000 or 10,000 years ago, I say that fact must stand against the whole science of geology. (Hear, hear.) Geology is not science; it is pure conjecture.

MR. HARTSHORN, of Ohio, U.S.—I have listened with great interest to the address which has been read to us, it being my business in America to teach natural science and geology. I do not intend to discuss the principles involved in the address at any length, but I wish simply to make one or two statements which may show in some degree how we stand in regard to this subject in the United States. I have conversed with many geologists on the Continent of Europe—in Germany, Russia, Austria, Italy, and France,—and I find that the best geologists and naturalists, whether believers in the Bible or not, generally admit that we have not as yet acquired sufficient geological data to justify us in all our conclusions. (Cheers.) The result is that we have different and often contradictory theories,—one theory to-day and another to-morrow; and in private conversation geologists will often admit frankly that which they will not put forward in their written works or in their public addresses. Many will acknowledge in the privacy of their own studies, that certain theories which they profess to hold have not yet been sufficiently tested by facts, and ought not to be taken for established science. Every geologist regards this subject of geology as in its infancy (hear, hear); and even the very best authorities that we have on the subject must acknowledge that certain positions which they now hold may be upset by facts which may come to light, and which may give a different direction to their present views. (Cheers.) I think those who believe in the Bible as the great chart leading to eternal life, need have no fear whatever with regard to geological discovery. (Cheers.) I am glad to see that the subject of geology possesses so much interest for the minds of Englishmen, and, I must say, I have never seen the subject so candidly, frankly, and truthfully approached as it has been to-night. I have no doubt at all that the future developments of natural science will only show that nature itself is but another page in the great volume of revelation. (Hear, hear.) Clergymen and Christians generally have an interest in this subject which no other people have, because they regard this earth simply as the handiwork and footstool of their Lord, and they feel they have a greater interest in becoming intimately acquainted with it than have other people. (Cheers.)

DR. GLADSTONE.—I have listened with very great interest to what has been said this evening. I was not present in Sion College when Professor Huxley delivered his address; but I have been told since, that the matter seemed to fall rather flatly upon that occasion, because, although he might have expected that what he had to say would be in opposition to the views of the main portion of his hearers, it did really appear that a majority of them were ready to go to a great extent with him. Professor Huxley said—and it must have occurred to most of his hearers to dispute it—that in discussing the result of scientific investigation with the received chronology of Genesis, he was assuming that there was a Biblical chronology which was generally

received by theologians. Now, I maintain that there is no such thing. I think a truer view of the question is to be found in Mr. Reddie's address, that we are not dealing with theories which have been drawn from ascertained facts. I need scarcely remind you that there are very great discrepancies between the different versions of the Biblical chronology—discrepancies amounting to 1,250 years, or thereabouts. The chronology of the Bible rests upon one genealogical table, to be found in Genesis, and referred to in 1st Chronicles and the Gospel of St. Luke. But we find it was the habit of the Scriptural writers to make large gaps in their genealogical tables. The first verse in the New Testament is an instance of this, for we find two generations mentioned stretching over a period of nineteen centuries. We know, also, that in the continuation of that chapter various gaps are designedly made in the genealogical table; and we can so trace the habit of those sacred writers in such cases that we are led to conclude, where we cannot apply a test at all, that the genealogical tables are incomplete. Very few who have looked into the subject will place such reliance on the common Biblical chronology as Professor Huxley seems to suppose, and I think the issue which he has raised fails on that point. It has been stated to-night that geology is not a science. I cannot accept that at all. Certainly it is not a science of the same kind as mathematics or chemistry; but I believe it is nevertheless a science, and one which may lead us to very decided conclusions. (Hear, hear.) I believe myself that man has existed upon the earth for a great deal longer than 6,000 years; but I believe, at the same time, that that is in no way opposed to any statement which I am called upon to believe in revelation. (Hear, hear.) I trust this discussion will be marked throughout with courtesy, so that we may not seem to be endeavouring to pit one class against another—to pit geologists against the clergy, for instance; and I hope we shall all consider that though we may differ from Professor Huxley's opinions, we ought to treat him courteously, and to consider his arguments and the whole question in all its bearings as becomes gentlemen and Christians. (Hear, hear.)

DR. HAUGHTON.—As one of the foundation members of this Institution, I may perhaps be excused for making a remark as to the position we occupy and with regard to the resumption of this debate. We claim to be a scientific Institution, and I trust that whatever remarks may be made, the speakers will distinctly keep that in view, especially when we remember the tone which has been adopted towards us by certain public journals of no small reputation, and among others, by the *Saturday Review*. It should be distinctly borne in mind that we claim to be a scientific Institution, and therefore, that the speakers should confine their remarks to the points of the discussion. (Hear, hear.)

THE CHAIRMAN.—Perhaps I may be allowed to make one or two remarks of a rather apologetic character for the subject which has been discussed this evening. It may not be generally known, even among the clergy, that the various meetings of Sion College, of which I am a fellow, were not express meetings of the fellows, nor are they convened by the Court of Sion College. They are convened entirely by the President of that body. The

President calls them, and the Court has nothing whatever to do with them, and the President invites whom he pleases to them. The gentleman who this year enjoys the privilege of being the President of Sion College thought it would be a revival of the meetings, which had been discontinued for some years, if he were to invite a number of eminent men to give papers upon different subjects. In the exercise of his own discretion he invited Professor Huxley, and, I suppose, allowed him to choose his own subject. Mr. Reddie, together with myself, was present on that evening; and I felt so strongly on the subject, after hearing Professor Huxley's address, that I asked to be allowed to make a reply, but was cut short by the President. I had followed up the attack by asking what it was that we were called upon to discuss? The subject, according to the programme, was:—"In opening the discussion on Thursday next, Professor Huxley will draw attention to the difference supposed to exist between scientific and clerical opinion, and inquire into the cogency of the arguments by which some scientific doctrines are supported." I complained that Professor Huxley did not tell the clergy what were his real opinions on these subjects, in the same manner in which he told them that no sensible man of science with whom he was acquainted, or any well-instructed person, believed in the ordinary chronology of the Bible. I denied that the clergy believed in the infallibility of Archbishop Usher's chronology; and I pointed out that elsewhere Professor Huxley had himself shown that there was a greater divergence between the opinions of scientific men than between the opinions of the clergy; and I brought forward a passage Professor Huxley had written and signed with his name in the *Fortnightly Review*, to prove this. The President, however, ruled that I was out of order in producing that which had been written by Professor Huxley elsewhere. I threw myself on the meeting, and said I thought I had a right to bring before my college brethren how great was the divergence between the opinions of scientific men, when Professor Huxley had himself stated in the *Fortnightly Review* that no man of science, and no well-instructed person, believed in the creation of Adam and Eve, using most offensive terms in doing so, and calling men who believed in that creation "Adamites, pure and simple." He there denied the special creation of Adam and Eve, because, he said, the very idea of creation itself was unphilosophical! (Laughter.) I pointed out that such diversities of opinion, sheltered under the name of scientific opinion, were far greater than those existing among the clergy, and I also pointed out that such differences were not simply differences between scientific men and clergymen, but between the faith of all Christendom and scientific men. (Hear, hear.) Mr. Reddie, like myself, felt strongly upon the subject, and he wrote to the President (understanding that there were only two meetings announced, and that we were promised other meetings after Christmas), to be allowed to reply to Professor Huxley at one of those future meetings. The President, however, wrote to tell him that the programme was filled up, and the whole of the lecturers appointed, and he therefore could not allow him to have the opportunity he desired. Mr. Reddie, however, felt that Professor Huxley had lectured the clergy in a rather

unmerciful fashion, with a quiet assertion that our Thirty-nine Articles were an impediment against our reception of truth, and that we were afraid of meeting the truth ; and he thought this Institution was one which would very likely afford him the opportunity of replying to the Professor, and that the clergy would be glad to hear what could be said on the other side. We invite here the fullest discussion and the most open debate, and I am only sorry that to-night the debate has been so one-sided. (Hear, hear.)

Rev. Dr. IRONS.—In consequence of what fell from Dr. Haughton, I would simply remind him that in the course of the remarks I was obliged to make as a clergyman, in consequence of the tone adopted by Professor Huxley towards Christianity and the Church, I did say, and now repeat calmly, that if Professor Huxley or any of his friends will put down in clear, distinct words what those hypotheses are which they conceive the clergy contradict, or are disinclined to adopt, I now pledge myself to consider every one of these publicly, and to give them either the fullest admission or the most unsparing exposure. (Hear, hear.)

The discussion was then adjourned until the next Ordinary Meeting, on Monday, January 6th, 1868.

ORDINARY MEETING, JANUARY 6TH, 1868.

THE REV. WALTER MITCHELL, M.A., VICE-PRESIDENT, IN
THE CHAIR.

The minutes of the previous meeting were read and confirmed.

It was announced that A. C. Brebner, Esq., Audit Office, Somerset House, had been elected as a 2nd class Associate of the Institute.

The following books were announced as presented to the Institute :—

- Shinar, the Scripture Record of The Confusion of Language*, and a new edition of *Adam and the Adamite*. By D. McCausland, Esq., M.V.I.
From the Author.
- Rule of Road at Sea* (three copies), by Thos. Gray, Esq., H.M.C.S., Mem. Vict. Inst.
From the Author.

The Discussion on Mr. REDDIE'S paper "On Geological Chronology, and the Cogency of the Arguments by which some Scientific Doctrines are supported in reply to Professor Huxley's Discourse delivered at Sion College on Nov. 21st, 1867)," was then resumed as follows :—

THE CHAIRMAN.—I have to invite you to the adjourned discussion of the paper by Mr. Reddie ; recently read and in doing so, I have to express the regret which I am sure is felt by all of us, that the great loss Mr. Reddie has sustained has prevented his being with us this evening, to hear and reply to the observations that may be made. (Hear, hear.) Of course we must reserve to him the privilege of replying on another occasion.* Mr. Reddie's paper, as you are aware, was brought before us under somewhat singular circumstances. Mr. Reddie had heard a lecture given by Professor Huxley at Sion College, upon the supposed discrepancies existing between the clergy and men

* *Vide* Note B, p. 373.

of science ; and, feeling that there was not a full opportunity given for the discussion of this subject at Sion College, and regarding it as one that was in consonance with the subjects discussed by this Society, while believing it to be of the greatest importance that the matter should be fully and thoroughly ventilated, Mr. Reddie thought it right to bring the question before this Society, and wrote the paper which will form the subject of this evening's discussion. I have now to invite further discussion on this paper, and as Dr. Gladstone told me on the last occasion that he wished to make some further observations on this subject, I am sure we shall be delighted to hear anything he has to say.

Dr. GLADSTONE.—I had no expectation of being called upon to open the discussion, and perhaps I am hardly prepared to do so. On the previous occasion, what I said bore upon this point, that in his lecture at Sion College, Professor Huxley enunciated certain views with regard to the antiquity of the earth and of man upon the earth ; and he expected that he was introducing something that would meet with a good deal of opposition, but found a large portion of the audience prepared to admit his conclusions on these points, and to think that there was nothing in them opposed to revelation. I expressed my opinion that there was nothing in those conclusions that Christians might not freely accept. I do not care at the present moment to go more fully into that argument, and if I offer a few remarks, I would rather offer them upon a larger issue—an issue which bears upon our practice as well as upon our belief. As I understand—for I was not present when Professor Huxley introduced the subject—he went to Sion College in the belief that the clergy, or that religious people were, upon the whole, rather opposed to science. (Hear, hear.) I believe that there is also a conviction existing in the minds of some other parties that, upon the whole, scientific men are rather opposed to religion. These two opinions are the converse of each other—and in fact a kind of polar antagonism ; and if I had an electric machine here, I could illustrate what I am saying by demonstrating how one body would become positively when the other is negatively electrified ; while the more strongly the one became positive the more strongly would the other become negative. I think that in society there is a great tendency to become polarized, and that the more strongly one set of opinions is insisted upon, the more strongly is another set of opinions enforced. I am afraid that sometimes we are disposed to fall into Professor Huxley's error. He thought there was this sort of difference between the scientific mode of thought and the theological mode——

The CHAIRMAN.—That is hardly so. Professor Huxley announced that he intended in his lecture to “draw attention to the difference supposed to exist between scientific and clerical opinion, and to inquire into the cogency of the arguments by which some scientific doctrines are supported.” The majority of his arguments he derived from geological evidence of the antiquity of the earth. He did not go into the question of the antiquity of man so much as the antiquity of the earth. He took the antiquity of man by the way, and then went into the general question of geological ages.

DR. GLADSTONE.—Well, Sir, I do not wish to enter into the geological question. I did not hear Professor Huxley, nor have I read his paper, and I believe there is a gentleman here who is better acquainted with the subject, and who will be able to show that Professor Huxley was, to a considerable extent, misunderstood ; or, in other words, that Mr. Reddie mistook Professor Huxley and his argument. But with this I have nothing to do, and I would rather say a few words with reference to the great question as to whether there is the discrepancy spoken of between scientific and clerical opinion. For my part, I do not believe that religious people or the clergy are opposed to science ; I think, on the contrary, we have abundant evidence that they are ready to open their minds to knowledge of every description, whether in connection with natural science or with any other subject. On the other hand, I do not believe that there is an opposition or antagonism on the part of the cultivators of natural science towards religion. I am frequently in the habit of meeting with scientific men, as well as with merchants, lawyers, and military and naval men ; and it is my belief that among scientific men there are just about the same proportions of real Christianity and of unbelief as are to be found in any other profession, while I am also of opinion that there is a much larger proportion of believing Christians in the ranks of science than in the ranks of many of our artisan trades. But when I have stated this, I am generally reminded that there are certain scientific men who are notorious infidels. This I grant ; but I say, if you will take any other profession, do you not also find many infidels in it ? There is, however, this difference between the two :—if a man be, let me say, a barrister, he cannot bring any arguments from his own profession against the truth of Christianity ; nor if he be an artisan, a cobbler, or a tailor, can he bring arguments from his craft against the truth of the Bible ; but, if he be a man of science, he can fall back on his profession, and can bring forward arguments opposed to some of the religious opinions of the day which he thinks are opposed to the statements of revelation itself. I think that this important difference ought always to be borne in mind. If you take the infidel barrister or cobbler, what does he do ? He cannot from his own profession or trade bring forward arguments against Christianity, but he goes to natural science, where he thinks he can find those arguments. It is a great deal, therefore, if such a man can only say that those persons who cultivate natural science are drawn by their studies into infidelity ; and thus we find that the orators who are to be met with on the platforms of the infidel halls of London are always ready enough and even rejoiced to maintain, true or false, that scientific men are, on the whole, rather inclined to infidelity. That in saying this they utter a calumny against the profession to which scientific men belong, I fully believe ; but, supposing it were the truth, would it not be a matter that we should mourn over in secret rather than be constantly repeating from pulpit and platform ? If this were the fact, it seems to me that it might be regarded as the strongest possible argument against the truth of Christianity. As I have said, I do not believe that the statement is true ; indeed, I am rather disposed to think that the truth is on the other

side ; but nevertheless what is said and insisted on in the infidel halls is frequently published in our churches and public assemblies. How easy it is to get into this antagonism, and to think on the one side that scientific men are not disposed to be religious, and on the other side that religious men are not disposed to be scientific ! I do not believe the statement either on the one side or the other ; and I think that we should do dishonour to God and an injury to our fellow-man if we admitted either proposition. I assure you it has often pained me deeply when I have been seated in a pew, and have heard from the pulpit without being able to say a word in reply, or I should have been brawling in church, the statement that there was this opposition between scientific men and revelation, and various things brought forward in which the preacher has shown the most lamentable ignorance as to scientific facts. I have heard preachers asserting that such and such things must be because the Bible says they are, and I have seen men listening to these statements, knowing very well that what the preachers have been inveighing against was actually true, and, of course, drawing the conclusion that, if the Bible is opposed to what they knew to be true, the Bible must be false. I have often felt that in such cases the preacher was doing the work of the infidel more effectually than the infidel himself. I thank you for the way in which you have received my remarks. You understand what I am striving against ; and, if I speak with warmth, it is because I feel what I say, knowing how apt Christian men are to fall into the error against which I would guard you. On the other hand, you ought to assert wherever you go that there is not the antagonism which has been supposed between scientific and clerical opinion, but that faith in the Bible and faith in natural science are perfectly compatible.

Rev. C. A. Row.—I thought on the last occasion that there was danger of this discussion becoming a wholly personal one, and that we were too much engaged with Professor Huxley, and too little with the facts of science and of the Bible. I regret that Mr. Reddie is not present on this occasion, because I had intended to make some remarks on the spirit which pervades a portion of his paper, and with which I cannot say I feel perfectly satisfied. I am convinced that he has treated the matter in a manner unsuited to us as a philosophical society, and I am afraid that, if this is done, we shall not attain much credit outside of this room. I have marked some passages to which I must make some reference. In the fourth page, speaking of the persecution of Socrates, he says :—"And who were his persecutors ? The professors of his day, who pretended to know everything, and went about giving lectures and teaching for profit their deleterious sophisms. I trust such a state of things is not in store for us !" Now, I have yet to learn that Socrates was put to death by the professors of his day. The persons who sought his death were the Sophists, and I am sorry that Mr. Reddie has sought to connect them with the modern professors. They were as much alike as chalk is to cheese. Those who have read Thirlwall's "History of Greece," and have studied Plato, will not draw the conclusion that the professors of his day were the persons who put Socrates to death. I am aware that Plato has made

reference in strong terms to their teaching for money, but I should like to know how, if they taught the common subjects of the day, they could have existed unless they had taken money for their teaching. I will now draw attention to another fact which I wish Mr. Reddie were here to explain. He has said that it is of great importance to quote Professor Huxley very closely, but he certainly does not do that. We ought to have most accurately the words of Professor Huxley—

Captain FISHBOURNE.—Mr. Reddie explained that it was a great misfortune that there was no reporter present, and also that Professor Huxley's lecture was not in writing.

Rev. C. A. Row.—At any rate, we are in danger of discussing what was not said. I will now draw attention to one paragraph of Mr. Reddie's paper as an instance of want of care and accuracy on his part. He says (speaking of Professor Huxley): "He afterwards quotes Herodotus as saying—'that this Nile valley was once a great arm of the sea, filled up in process of time by mud brought down by the Nile—this great Nile valley, 1,200 miles long—filled up by mud forced down the Nile. And unless you are prepared to deny this condition of things, that in the time of Joseph, and long before, this Nile valley must have been essentially what it is now, ask yourselves what period of time this process of filling up this huge arm of the sea must have taken.'" Mr. Reddie quotes Professor Huxley as stating that Herodotus used these words, but I do not believe that Professor Huxley said anything of the kind. In fact, I would rather believe in the miracle of Januarius's blood than that Professor Huxley ever made such a statement. Can he believe for a moment that Joseph is mentioned in the second book of Herodotus? I have been for many years head master of a grammar-school; and, if any one of my boys had made such a statement in answer to a question, I should have made him write out the whole of the book until he met with the name of Joseph: and he certainly would have had to write the book to the end—

The CHAIRMAN.—What is quoted as having come from Professor Huxley is taken from the notes of a clergyman who was present, and I believe they were considered to be extremely accurate—

Rev. C. A. Row.—I cannot believe that Professor Huxley ever uttered such a piece of abominable nonsense—

The CHAIRMAN.—It is not said that he did mention the name of Joseph as occurring in Herodotus.

Rev. C. A. Row.—That, at any rate, is the meaning of the sentence from its grammatical construction—

Dr. IRONS.—Are we not quibbling about words? There is not any pretence for imagining that in that passage Joseph is mentioned as being alluded to by Herodotus.

Captain FISHBOURNE.—I was present, and I know what is intended by that passage. Mr. Reddie means that Professor Huxley quoted Herodotus as saying that the valley was filled up by mud brought down by the Nile at the time in which Joseph lived, but without mentioning the name of Joseph.

Dr. IRONS.—That is clearly the meaning to be attached to the passage. Mr. Reddie refers to the time of Joseph, and not to any mention of Joseph by Herodotus.

The CHAIRMAN.—I think, if Mr. Row takes the words as they stand, he will see that it was not intended to say that Joseph's name appeared in Herodotus. It may have been a slip on the part of Mr. Reddie in not having used inverted commas.

Rev. C. A. Row.—At any rate, I consider it was a mistake, and when we are discussing subjects of this sort we ought to be particularly accurate. However, I will leave the personal question, and will proceed to draw attention to some quotations from Professor Huxley upon which some remarks have been made. I find it exceedingly difficult to derive any theological or scientific issue from the paper of Mr. Reddie. He does not state whether he thinks the world 6,000 years old or a few thousand years older; but I suppose it may be taken as a general inference that he wishes to argue that it is only 6,000 years old. If he sets that up distinctly, I could beat him upon that issue by saying that there is nothing in the Bible directly or indirectly asserting anything of the kind. I wholly deny anything approaching to the chronology of the Bible as it has been stated. Take, for instance, the first chapter of St. Matthew. It is there stated in the genealogy given that between so and so and so and so there were fourteen generations. How is this made out? In one instance it is done by leaving out the names of no less than three kings—Ahaziah and his two successors. If this be the case in the genealogy given in the New Testament it is reasonable to ask why should not similar omissions be found in the Old Testament?—

The CHAIRMAN.—They are.

Rev. C. A. Row.—If the New Testament is, as I apprehend, written under a far higher inspiration than the Old—(Cries of “No, no.”) However, that is my opinion. I have written a work on the subject in which any one may see what my opinion is, and it is my decided opinion that the spirit of inspiration in Our Lord and the Apostles was higher than that which dwelt in any prophet whatever. We are raising false issues in this matter. I have brought with me a book with which I have no doubt my reverend friends are very well acquainted—namely Paley's “Evidences of Christianity.” Archdeacon Paley there says: “Undoubtedly, also, our Saviour recognizes the prophetic character of many of their ancient writers. So far, therefore, we are bound as Christians to go. But to make Christianity answerable with its life, for the circumstantial truth of each separate passage of the Old Testament, the genuineness of every book, the information, fidelity, and judgment of every writer in it, is to bring, I will not say great, but unnecessary difficulties, into the whole system.” This book is itself endorsed by the Church of England—

The CHAIRMAN.—I must protest against that. A book may be a text-book without its being endorsed by the Church. I don't think the fact of its being taken as a text-book pledges one to the perfect orthodoxy of every passage.

Rev. C. A. Row.—I have merely introduced this to show that the attempt

to set up a system of chronology as part and parcel of Divine revelation is entirely out of the question. No one could defend at the same moment the shores of an entire kingdom. Even the Duke of Wellington, if, instead of taking his position within the lines of Torres Vedras, had attempted to defend the whole Peninsula of Spain, would necessarily have been driven out. I, therefore, take my position in my Torres Vedras, and no one shall compel me to fight outside of it, and I will not consent that persons should imply that certain things are necessarily the subject of Divine inspiration which, as far as I can see, lie entirely beyond the sphere of that inspiration. I wish now to draw attention to the subject of the civilization of Egypt. Professor Huxley recites the fact of that civilization, and alludes to the chariots and horses and so forth of Pharaoh's time, resting the fact of Egyptian civilization pretty much upon those trivial things. If I walk into the Museum I see there a great mass of evidences of Egyptian civilization, and that is what I wish to account for. In doing so, I am met by a difficulty as to the origin of man—whether he began as a savage, which I do not believe, or whether he was originally in a high form of civilization. I find no chronology which will account for the formation of the high state of civilization at that extremely early period, and in saying this, I am the more concerned with the system of religious worship which belonged to the Egyptians. It would be exceedingly difficult to account for the progress made by the Egyptians, in these few centuries, in arts and sciences. And if you go to India and see the early civilization evidenced there, and in China also, the difficulty is by no means diminished. All these things must be accounted for; and if the Bible does not impose upon us the necessity of saying so, why should it be asserted that the whole of these things must be accounted for in the period of 4,000 years that has elapsed since the Flood?—

The CHAIRMAN.—One of the lessons which Professor Huxley learnt at Sion College was that none of the clergy were prepared to maintain the infallibility of Archbishop Usher's chronology, and that is a fact which Mr. Reddie carries throughout his paper. There is no doubt that there ought to be a large extension of the present chronology. That no one denies. What Mr. Reddie has taken up is the enormous period which Professor Huxley required—something like millions of years, instead of 6,000.

Rev. C. A. Row.—But he does not require it as the period assigned to the human race.

The CHAIRMAN.—Yes. For instance, in the case of the Nile, the little arithmetical sum which Professor Huxley favoured the clergy with, gave something like 7,000 years for the formation of the Delta by the mud of the Nile, and he left us to imagine how much more ancient than that man may have been. Professor Huxley carries man back as far as the Tertiary period; and from what we have got from him and other scientific men in reference to the chronology of the Tertiary period, there is little doubt that both he and Sir Charles Lyell would be prepared to maintain that the chronology of man extended over millions of years.

Rev. C. A. Row.—But there is nothing of that kind in Professor Huxley's

lecture. He certainly laid down the proposition that the civilization of Egypt implied a longer chronology than many persons admitted——

The CHAIRMAN.—Upon that I think there can be no dispute.

Rev. C. A. Row.—But the question is a much longer one than that, and if we go thoroughly into it we must go into the origin of the Indian civilization and of their religion, and the origin also of the civilization of China and its religion ; the time it took to create those things, and various other questions. I am not prepared to say how long a time these things must have taken, but I use them in order to caution you against laying down a strong and limited Biblical chronology. I consider that there is great difficulty in accounting for the Egyptian religion, which must have grown up in the existing Nile valley, because that religion is deeply stamped in certain parts with the scenery of Egypt. If we assume that man was created as a savage, this supposition involves a more extended idea of the miraculous than the other. Certainly it would take a very indefinite period of years to raise man from a savage to anything like the civilization of Egypt ; but when I view the peculiar form of the civilization of Egypt and of the Egyptian religion, I say it is a difficult matter to state how long it must have taken in its elaboration. I should think—but I am speaking on entirely human ideas of chronology—that the growth of such a religion, infinitely and vastly complicated as it is, would take a very considerable interval ; and then we have also to account for the origin of the Egyptian language, and to go further, and to find the science of language rapidly springing up around. All of this would make a large demand upon time ; therefore we have to act with great caution before putting before the public any idea as to 6 or 10 or 20,000 years being the chronology of Divine revelation. At the same time, Divine revelation was not given to teach chronology or science. There are one or two other points in Mr. Reddie's paper which I regard as taking up a somewhat questionable position, but I will not enter upon them now. I cannot help saying, however, that I think his conclusions respecting the chalk formation occupying a period of only something like a century is an exceedingly questionable one. I admit that the result might be shown by the figures he employs, but in the same way I might quote the old sum, showing that a farthing put out at compound interest at the creation of the world would become so large a sum that it would have made a mass of gold as big as the globe. Of course, every one knows that that would be impossible, for there is not enough gold to do it, and I am afraid that we should have to resort to an inconvertible mass of bank notes.

Professor MORRIS.—I am sorry that I am in a worse position than either of the two previous speakers, inasmuch as I neither heard Professor Huxley's lecture, nor have I yet read Mr. Reddie's paper. All the advantage I have derived was the pleasure of attending the last meeting at which that paper was read, and I only recollect a few points of the subject under discussion. In the many observations that were made at that meeting I am glad that one speaker took the scientific part which was most ably defended by Dr.

Gladstone, whose remarks on that occasion, I think, were even more pungent than they have been to-night. I found that there was a tendency on that occasion, though perhaps unintentionally on the part of the speakers, to throw great blame upon scientific men. One gentleman frankly stated that geology was no science, and at the same time he admitted that he knew nothing about it. There was another gentleman who, I believe, is in this room at the present moment, who threw a rather hard taunt at scientific men when he said they were not acquainted with works upon theology, but that the clergy were deeply acquainted with works upon science. But these matters are somewhat apart from the very able paper of Mr. Reddie. Whatever may have been said by Professor Huxley,—and I am not here for the purpose of defending him,—I only wish he were present himself,—I am satisfied that he must have been to some extent largely misunderstood, and I am certain from what I know of him—of his strong cast of mind and straightforward integrity of purpose—that there are few men who would more readily give way to argument than himself, as he is at all times open to conviction. Having said so much, I will come at once to the moot question of the Nile Valley. I speak advisedly when I say that he only incidentally alluded to the accumulations of the Nile Valley. If I enter into that special portion of the argument which was more or less entered into upon the previous occasion, I hope this Society will bear with me, as I shall do so with a view of endeavouring to explain what the geologist really wishes to expound. There were sundry statements made the other evening with regard to the time required for the accumulation of different formations, and certain allusions were made to points in connection with the physical history of the Nile Valley, and the existence there of a peculiar group of rocks known as the Nummulitic Limestone Rocks, of which, as some of the clergy may know, the Pyramids of Egypt were constructed. This band of rocks belong to a group of formations comparatively modern in the geological history of the world. It ranges from the Bay of Biscay to Central India, and also reaches into the Chinese Seas. These rocks belong to the Tertiary age, but to the older Tertiary period, and lie just above what is known as the Chalk Formation. Here, then, you have in the Nile Valley a series of deposits of some comparative antiquity. I say comparatively old, because they represent one part of the Tertiary period, and from the allusions he made to the existence of man in the Tertiary period, I am sure that the Chairman, at least, who knows so well what the evidences are, would not be inclined to put the date of man's existence as far back as the older Tertiary period. Then we have the sea deposits—the rocks of the Miocene and Pliocene periods. Since the formation of the nummulitic rocks, a great part of the land of Europe has been added, and all the large cities stand on strata which formed at one time the bed of the ocean since the deposit of the nummulitic rocks. This then is an argument of some force with reference to the Nile Valley. Again, it is pretty certain that the origin of the delta of the Nile is the drift of the Nile, though I am quite aware, in reference to this fact, that it may tell as much against the geologist as for him. We have no right to measure by what we now know, the bringing down of the sandy matter, and depositing

it as a delta, or in the sea ; we have no right to measure by the present rate, what was the former rate of this deposition, knowing what we do of the upper part of the Nile Valley, a great portion of which consists of a stratum that is not very easily decomposed. It may have been that, in earlier times, a larger amount of mineral matter was brought down in deposits than at the present moment. This then is an element of caution of which geologists should take heed. There is, however, another point, and that is the Chalk Formation ; but I am not prepared to argue that. Still I think that Mr. Reddie must have misunderstood Professor Huxley. He says, "Look at the white chalk. It reaches a thickness of something like 1,200 feet, and is almost all composed of white matter. It is made up of a large assemblage of broken fragments, the remains of shell-fish, corals, and other forms of life existing in the seas at that time." Mr. Reddie assumes that the chalk deposit could have been accumulated in less than 100 years. If we take our present evidence—and I only argue from that—we find that in the bed of the Atlantic, in the deep-sea bottom, the mud which is brought up—and we have only been able to penetrate that mud to a depth of 14 or 15 feet—somewhat represents, when dried, a similar quantity of chalk. It is found to be composed of from 90 to 95 per cent. of foraminifera, which live in the Atlantic. This is an instructive fact which science has brought to bear on the history of bygone ages,—95 per cent. of these small animals having left their exuvæ. If any one will take the trouble to wash a piece of chalk with a brush, he will find similar organisms, some of which cannot be separated from the existing species. If then it be the case that only 14 or 15 feet of the Atlantic deposit have been accumulated within 6,000 years, I think it must raise a doubt as to whether the existing chalk formation has not taken a much longer period——

The CHAIRMAN.—May I ask, do you say that we now know the depth of the bed of the Atlantic to the extent of 14 feet ?

Professor MORRIS.—That is all that we have at present arrived at——

The CHAIRMAN.—How have we arrived at that ? From all I have heard of the deep-sea soundings of the Atlantic bed, the mud brought up from a depth of $2\frac{1}{2}$ miles is merely scooped up by a little apparatus attached to the heavy weight that carries the soundings. I was not aware that we had gone to such a depth as you state.

Professor MORRIS.—I believe that in some of the very heavy soundings they have gone to that depth. Disregarding this statement, the chalk formation, from the mineral character and organic remains, must have taken a very long time for its deposition. While there is no doubt that we clearly understand the origin of our chalk from studying present deposits, we must take into consideration the fact that the thousand feet of chalk contain also other organisms, of which we find no traces in the present seas. It is said that that chalk must have been accumulated as shallow deposits, inasmuch as a large number of the fish, corals, and so on, whose remains it contains, could not have lived at a very great depth. We have been told of star-fishes adhering to the rope which has been let down to the depth of a mile and a half ; and the evidence of that is somewhat suggestive. I will pass now

to another point. Mr. Reddie, in his paper, seems not clearly to have understood Professor Huxley with reference to the time in the earth's history required for the formation of the separate strata. Professor Huxley alluded to 29 or 30 distinct formations which constitute the superficial material of the earth's crust. All these, I should like it to be understood, indicate as many separate layers, marked by distinct assemblages of organic remains, very few of which ever passed from one of these layers into the other. This may be taken as one proof that the stratified rocks were not accumulated at one period. It was formerly thought that there were great gaps or breaks existing between these formations. We are well aware that these 29 or 30 layers are divided into three great groups, known as the first, second, and third life periods. But so marked are they that any ordinary person with the least instruction could at once say, on seeing any portion of them, brought from any part of the world, to what period they belonged. That there were passages I am willing to admit, between what we used formerly to think the great breaks in time between the primary and secondary periods. Throughout all periods of the earth's history there have been breaks. One group of rocks lies upon another, not in the same parallel direction, and the two are said to be unconformable. While we find that in this country there is a decided break between the organic forms of the oldest rocks and the overlying strata, and no continuity of life, in the Tyrol we find in the Trias there is an assemblage of fossils, some genera apparently belonging to the old period, and some apparently belonging to the secondary period. Taking these things into consideration, I think that those persons who look into the question must at least allow that there is some argument for time. For instance, if we look at past ages, how is it that we find no remains of man or his works in these deposits? Professor Huxley traced these remains back to the latest Tertiary period. That, of course, even would require great lapse of time. You have all the accumulation of the London, Paris, and Berlin basins belonging to the earliest part of that period. You have all the forms distinct from the present period. There is another group in the Miocene beds, and the remains are still different from the existing forms, and then you come to the more recent accumulations connected with or overlying the glacial drift alluded to by Dr. Buckland. It is only from these accumulations in the valley of the Somme and elsewhere that we get the first traces of human works, in the shape of flint knives. Beyond this, in the older periods, we get no evidences of man's remains. There are none amongst the Silurian rocks—no fragment of a canoe wrecked among the coral reefs of the Carboniferous period; there are none among the plants of the Coal period; none among the Saurian bones of the Lias. These facts, I have no doubt, many of you have read in the admirable pamphlet of Mr. Pattison published under the title of "New Facts and Old Records," a work which I would recommend to the study of those who have not seen it. If I may say a few words more, I can only regret the remarks which have fallen from one speaker as to men of science not being read in theological works; and have much pleasure in mentioning a few names to whom geologists are

greatly indebted for their testimony in reference to geological science. I find in the earliest works of Bishop Sumner—his “Records of Creation”—some admirable remarks which fully bear me out. I turn to Cardinal Wiseman’s Lectures on Religion and Science, and I find that he speaks upon the intimate connection which exists between geology and revelation. I may refer also to Martineau’s lectures, and to the eloquent discourse of Professor Sedgwick, “On the Studies of the University of Cambridge.” I find in all these that much is said of the true connection between religion and science. When I turn to Dr. Melville, I find that he furnishes a further evidence of the respect with which the leading men amongst the clergy have regarded the doctrines of science. Let us remember, then, that there are clergymen who have given ample testimony to the relations between science and theology, and, as has been said, we ought never to shut our eyes to the onward progress of science ; for if theology is not able to keep pace with philosophy, it will hardly be able to cope with infidelity.

Rev. Dr. IRONS.—I have been somewhat surprised to have heard it stated that the clergy shrink from science. I have previously affirmed the opposite of that proposition, and I again affirm that the clergy have evinced a strong love of science ; and the names just mentioned by Professors Morris might have appeared in the address which I delivered at Sion College, and which Professor Huxley seemed to think very *apropos* to the whole question. But this question has assumed, though through no fault of ours, somewhat of a personal character. There is no doubt that it was brought forward in an unfortunate way by Professor Huxley. In the presence of about 200 clergy and laity he charged us as a body with being obstructives and opposed to the course of science. I think I demonstrated on that occasion that the very opposite of that statement was the truth ; but I admitted also, that it was a most unfortunate issue—that it was a most unhappy thing for scientific men to quarrel with us, and that it was an equally unhappy thing for us to quarrel with scientific men. I also wholly demur to what has been stated by Dr. Gladstone, who tells us he hears clergymen make statements from the pulpit on scientific matters, which could be contradicted by young men, who hear them and know them to be utterly opposed to the plainest facts, and to truth. I can only say that I am a little older than Dr. Gladstone, and that I never heard such a thing in my life. I do not believe that the clergy are in the habit of doing so, and I never heard it imputed to them, except by scientific men, who, I again say, do not read our side of the question. I never knew a scientific man who studied theology. The men whom Professor Morris has mentioned were theologians who studied science, but I have never known a purely scientific man who had studied theology at all. I have in conversation examined not a few of them quite as closely as I should wish to be examined by them, and I publicly repeat, knowing that my words will be taken down, that scientific men who are not theologians by profession are not in the habit of reading our side of the question. With respect to the question of the antiquity of man and the antiquity of the world, the clergy do not flinch from going into that question ; it is the scientific men who

flinch from stating what they believe. We are quite ready—I am at this moment—to say what we think and what theologians are allowed to think, because theologians are liberal to one another and to scientific men, although they themselves are not treated with liberality. I am prepared now to say what I think as to the demand made upon our faith by the Bible in this question of man's antiquity, and I wholly deny that there is any fixed chronology laid down for us in the Scriptures. There is no reason in the world why every statement of Professor Huxley on this head—so far, of course, as it was an accurate statement—should not be admitted by us; there is nothing that I know of in Holy Scripture to prevent us giving the geologists, if they please, the first verse of the first chapter of *Genesis* to build their millions and millions of ages upon. I am not saying that I concede, or that I deny, in this matter. I am quite prepared to admit the great ignorance of scientific men at the present day as to the antiquity of the world, and I am sure that Professor Morris will not refuse to admit this also. What I regret is, that we are constantly assailing each other, as if each suspected the other of dishonesty. It is this that I protest against. Why cannot scientific men honestly advance their cause to us, exploring facts and shrinking from nothing which comes before them, without imputing to us any reluctance? Why cannot they believe that theology does not consist in a blind narrowness; that there is something more in the theological world at the present day, and has been in God's Church from the days of St. Augustine: that there is, and has been, a high tone of penetrating rational theology pervading the Church in all ages, and that we are not afraid of it now? I should be glad if there were this issue to our discussion, that scientific men would understand what they were about, and that the clergy would separate themselves more and more from the narrowness of sectarianism on this question. I do not think that any question has been raised in our time of more vital concern to the progress of Christianity than that which has been incidentally started in consequence of Professor Huxley's attack upon us. It is very easy to say, as Professor Morris has said, that Mr. Reddie has misunderstood Professor Huxley; but in answer to that, I must say that Professor Huxley is one of the clearest speakers I ever heard—a man not easily misunderstood—a man not carried away by any hasty enthusiasm—a cool-headed, prudent, and thoughtful man. I wished to do him all the justice in the world; but I stood up at Sion College and said, although it was rather late, I desired to ask the President whether he would permit me to ask Professor Huxley if he meant to say that the clergy he was addressing were rogues or fools. I did not impute to him the use of such rough words, but I said I liked to translate platitudes into the simple English of common sense, and what Professor Huxley had said left on my mind, and I believed had left on the mind of every person in the room, the impression either that the clergy shut their eyes to the facts of science, or that they were so densely stupid that they did not comprehend those facts; in fact, Professor Huxley had made it out that they were either fools or knaves. Professor Huxley immediately rose, and said, "I beg your pardon;

I said nothing of the kind." I asked him, "Was that your meaning?" and he replied, "Not in the least." I said, "Thank you. Your whole speech means nothing, if it does not mean that: it had no other sense." Now, I complain that there should be this imagined antagonism between men who are purely following science and men who are following religion. Why, I ask, should they imagine themselves to be antagonists when they are not? We are both serving the same sacred cause of truth, and if I found a scientific man thinking entirely opposite thoughts to my own, but still thinking and expressing them honestly, I would grasp his hand as a fellow-worker in the mine of truth; but if he were to turn round on me and say I was not hearty in the cause, that I did not pursue truth as he did, I should think he was a somewhat mistaken man in that particular, however honest he might be in his own pursuit. I think I have occupied quite enough of your time: what I have said has been in self-defence;—I do not mean of myself personally, but of the clergy as a body. We all of us read the scientific books as they come out. I have them on my own table, and I find them on the tables of my brethren; but we do not find on the tables of purely scientific men the latest theological works. Let us, I say, endeavour to get rid of the petty spirit I have referred to, and work together as brethren in the great cause of advancing truth.

REV. S. WAINWRIGHT.—I take a position midway between that of Dr. Irons and that of Professor Morris, but one occupying ground common to both. I sympathize with the remark made by Professor Morris in reprehending the observations made by one gentleman, who on a former occasion began by saying he knew nothing of geology, and then asserted that geology was no science; but although I regard the statement as indefensible, I think it was not an unnatural remark on the part of a person having probably only a superficial knowledge; and at the same time I hold that there is something to be said for what that gentleman meant. I understood him to say that geology differed essentially from the stricter sciences in the method by which geologists arrive at results, and I regard this as a proper distinction, and one that may be fairly maintained. With respect to Dr. Gladstone, though I think his main proposition utterly indefensible, I do not mean to say that I differ from him *in toto cælo*. I understood Dr. Gladstone to say, on the last occasion, that there was nothing in the Bible approaching to a chronology, and on the present occasion that statement has been most emphatically repeated by Mr. Row—

REV. C. A. ROW.—I doubt whether there are data in the Bible on which a positive chronology can be constructed.

REV. S. WAINWRIGHT.—Mr. Row stated that we had nothing approaching to a chronology in the Bible, and that proposition I deny as emphatically as it is affirmed by Mr. Row. I say deliberately, and after mature consideration, that the whole controversy as it exists to-day between true science and religion—and here I fear I am using a terminology requiring considerable forbearance, because you cannot separate the two things, inasmuch as true science involves religion, and religion involves true science—the

whole controversy, I say, between science and what is commonly called religion, arises on the side of *quasi* religious people, from the habit of confounding the facts of religion with a mass of interpretations and theories founded upon certain other facts; and on the side of science, I submit that the same process of error obtains to no less an extent. I say, unhesitatingly, that we have in the first chapter of St. Matthew something approaching to a chronology. I freely admit that in one place there are three kings' names omitted, and that two are omitted in another; and I also admit the force of the inference that if you have gaps of this sort in the New Testament you may find, but not *must* find, corresponding gaps in the Old Testament. As an instance, we have the names of Eber, Peleg, and Reu, three patriarchs coming in consecutive order in the Old Testament. Eber is mentioned as having lived 464 years, and Peleg only 239. It is pointed out that this disparity of age indicates probably a gap; that is to say, that if we had all the names from which that register was compiled, we might find some gap similar to those in the first chapter of Matthew. But my point is, that these irregularities are like the irregularities in the grammar—they follow a rule of their own, and if we have here and there a clue, whether it be unique or manifold, by which to estimate the duration of the gap, we are thus furnished with the means of completing to even greater perfection the chronology which I maintain we have without the clue. In the Acts of the Apostles, St. Paul, immediately after his conversion, began to preach at Damascus; a riot was the consequence, and his life was endangered, but he escaped and went up to Jerusalem. But in St. Paul's own account of the matter, given in his Epistle to the Galatians, he says he left and went to Arabia, and was there three years. You may say, "How is it that the two accounts clash?" The fact is that they do not clash; the historian has simply related those things with which he had immediately to do, whatever else is wanting being inserted in its proper place. I say that these irregularities have a rule of their own, and that they constitute a means of measuring the chronology of the Bible. I do not say this because I am anxious to defend the popular chronology of 6,000 years;—it may not be in the Bible, but I am careful not to say that it is not in the Bible, and the day may come when our grandchildren will see that it is there as clear as noon-day. I admit that there is an important distinction between the facts of Scripture, whether chronological or otherwise, and the inferences that may be based upon them; but I ask, is this a danger that pertains to religious men? No, it is a danger that exists on the other side. In defence of Mr. Reddie, the cause of whose absence I am sure we all most deeply regret, I will say a few words. Professor Huxley is stated to have said: "You [the clergy] tell your congregations that the world was made 6,000 years ago in six days, and that all living animals were made within that period." We have it in the report of the Norwich Congress that Dr. Pusey distinctly stated that the clergy never said anything of the sort. In the first part of Genesis it is not asserted that the world was made or created in any definite period of time; but what is said may be taken as a distinct declaration on the point that has been

raised, as to whether matter was eternal and whether it could come of nothing. This is the point to which St. Paul referred when he said that the world was made by the word of God, and not out of the things which now appear. What the six days may mean I will not go into now. I am willing to take them either as six days or a more lengthened period if you like ; but I say that the narrative is so worded that it may be taken either way. When, however, men come to us with their own interpretations and tell us they are men of large and expanded opinions, I think we have a right to ask them, "Where are your facts ? Is it not the mark of science that you should take your stand upon your facts ; like Newton, when he refused to believe certain things about the satellites of Jupiter, because he had never seen satellites having the characteristics described ?"

Captain FISHBOURNE.—I may state in reference to Mr. Reddie, as he is unable to be present, that he specially asked Professor Huxley to come forward, and therefore the absence of Professor Huxley was his own fault.

Dr. GLADSTONE.—Mr. Wainwright seems to think he differs from what I have said. I feel it incumbent on me to say that I go entirely with him as far as the chronology of the Scriptures is concerned. No doubt there is an apparent chronology, but it appears to me, if we look more minutely into that chronology, we find that there are important omissions, and we do not know, especially in the more ancient records, to what extent those omissions may have gone. We find in the account of the dispersion of the tribes of man after the Deluge, that some nations are said to beget other nations. Now, when that is the case, it is open to us to think that the antediluvians alluded to as having lived for 900 years may have been nations or dynasties instead of individuals. This would show that we are not able to form any definite opinion, within a few thousand years, as to what the antiquity of Adam might be. That we must draw on the Bank of Time for a much longer period than 6,000 years is evident to me from the convergence of a number of arguments. There is the geological argument—the argument of finding the works of man in comparatively ancient drifts ; for instance, in the valley of the Somme—an argument which, I think, obliges us to give a greater antiquity than 6,000 years. Then there are other discoveries made in Switzerland and Denmark ; and beyond this, there is evidence that man coexisted along with, not one species only, but many species of animals which are now totally extinct in this part of the world. Again, if we are to suppose the languages of the earth to spring from one stock, we must require a much longer period than 6,000 years for that. Then we have the ethnological argument. If we believe, as the Scriptures require us, that man sprang from one single pair, then apparently we require a much longer period than 6,000 years to account for the large divergence of race which now exists. I think, also, various histories of ancient nations, with their civilization and religion, require an extension of that period. I do not say that any one of these arguments is conclusive in itself, but I contend that, combined, they afford a very strong proof that man has been upon the face of the earth for a longer period than we have generally attributed to him ; but

at the same time I do not go to the extent of believing in the hundreds of thousands of years which some of our friends speak about. It is assumed that I have stated that Professor Huxley had been misunderstood by Mr. Reddie. I did not mean to say so with regard to the main scope and purport of Professor Huxley's address, but merely as to some of its details.

Rev. S. WAINWRIGHT.—I merely wish to say one word. You have just heard from half a dozen sorts of evidences how likely it is that the human race is older than 6,000 years; I only want to mention two facts to show with what caution that evidence should be received. The Valley of the Somme has been referred to. The theory there is that the river must have been at one time a mile in breadth to have filled up the present valley with the drift found there. Now, when the river was a mile in breadth, was it not diminished in force so as to have been unable to do what at its present width it might be supposed to do? There is also another theory as to the elevation of the river's bed; but into that I will not enter. The other point is as to the date said to be required for the formation of peat moss. It is said that 4,000 years is the lowest period, and some even go as far as 16,000; but it has been shown that in Ross-shire eight feet of peat moss was actually grown in fifty-eight years.

Captain FISHBOURNE.—I only wish to say a few words with regard to what has been said about the want of a proper spirit in Mr. Reddie's paper. These remarks could only have arisen from the fact that the gentlemen who made them did not hear Professor Huxley's address, and were unaware of the spirit in which it was delivered. Perhaps I may speak upon this point with more propriety than any clergyman. I was there, and heard Professor Huxley allude to the narrowness of the view taken by clergymen, whom he assumed to be bound by the Thirty-nine Articles. The impression on my mind was that it was one of the most insulting addresses I ever heard. He stopped short by saying that he would and could have said more had he spoken the whole truth, but he did not like to do so, as it might be regarded as insulting to the clergymen whose guest he was. That pointed to a different issue, and although his statement did not precisely indicate what that issue was, he left us to infer it. Hence the question of Dr. Irons as to whether the clergy were knaves or fools. This being the case, I do not think it can be said that Mr. Reddie's paper is not written in a proper spirit. One speaker has said that Professor Huxley's paper does not raise the question of the inspiration of the Scriptures. The fact is that Mr. Reddie has followed out more strictly the scientific points, though Professor Huxley did raise a larger issue. That he intended to raise the whole religious question is evident, for, speaking of science, he said "that is my religion. Call it fanaticism if you will. We are utterly at issue with you clergymen, and, of course, with your religion. Ours is demonstrably true, and you will have to give up yours and come to us." This being the case, I cannot understand how Mr. Reddie's paper can be characterized as wanting in proper spirit or tone; and I am only surprised that he could have written as temperately as he has done. I certainly could not, with any regard to what I felt,

have written or spoken in so temperate a manner. But even had I written the paper, I should not have asserted that all scientific men were opposed to religion. My friend Dr. Gladstone is a scientific man, and so far from his being opposed to religion, I know that a great part of his life is given to the propagation of the truth; therefore I think we must make a distinction, and the distinction is an important one; but there is a class of scientific men who make a boast that they have no religion, such as we understand it, and if these men give a character to their body, it is they only who should be found fault with by the religious portion of their profession, and not us.

Rev. C. A. Row.—I do still feel that somewhat strong language has been used, and I think that if Professor Huxley did revile, it is our duty not to revile again, and the less fuss the clergy make about this matter the better it will be, for people will otherwise say that the cap fits us.

The CHAIRMAN.—I will now say a few words by way of summing up, and they shall only be few—not because I have little to say upon this subject, but because it is so large and important that I could hardly do justice to it in the time which remains to me, and because, also, I hope to speak upon it in another place. I think that a great deal of misapprehension has arisen from the unfortunate nature of the discussion which has taken place upon this subject—unfortunate, not so much in the way in which it was taken up by Mr. Reddie, as in the manner in which Professor Huxley thought fit to instruct the clergy. And here I must say that I think there has always been great caution displayed by this Society in its method of procedure. I believe there is no real discrepancy at all between those who believe in revelation and those who make real science their chief study. I wish you to understand that by real science I mean that which is demonstrated to be true. I believe that where discrepancies do arise—and doubtless great discrepancies and discordances exist—they arise, not with regard to that science which can be demonstrated to be true, but from those floating hypotheses of science which are held to-day and contradicted to-morrow. (Hear, hear.) I think there has been a total misapprehension as to what was the nature of Professor Huxley's discourse. He did not go to Sion College to read a paper on a particular and definite subject; but he went there to open a discussion—a particular and definite discussion—and he was afraid, when he came before the clergy, to meet them upon fair grounds. He ought to have known, if he had known anything at all of the opinions of the clergy, what has been stated by clergymen here this evening, and what he was told by some who spoke upon that occasion, namely, that those who had not followed the progress of science would have allowed him pretty nearly everything he asked. But he did not enter into the real history of the alleged divergence between the clergy and men of science. I will here call attention to what is certainly one great divergence; and that is, when men deny the fact of creation—when they deny the unity of the human race. Make what you will of Christianity, you cannot get away from the fact that if you are to give any honest interpretation of what you believe to be a divine record, divinely inspired,—if you are to take the

New Testament, and believe it to be inspired,—you must also believe that in Adam all died, and that the unity of the human race is a thing essentially connected with the revealed doctrines of Christianity. (Hear, hear.) When a scientific man denies this, then I say that there is such a divergence between scientific and theological opinion that it is necessary—I will not say at once to tell that scientific man he is absolutely wrong in his conclusions—but at least to ask him to examine very narrowly the evidence upon which he makes that assertion. (Hear, hear.) Now, Professor Tyndall, in somewhat offensive terms—for I regard them as offensive terms to use towards a body of clergymen—has talked about the necessity of men having exact education in the exact sciences. I should like to know where you will find throughout Europe a body of men, taking them generally, who have had a more exact scientific training than the clergy of this country. (Hear, hear.) I will take the case of the University of Cambridge; and I have no doubt that Dr. Irons will make the same plea on behalf of Oxford. When the French philosophers came over to this country and heard the answers given by our young men at Cambridge to the questions put to them in pure science, demonstrative and mathematical science, they wanted to know what became of all these philosophers. I maintain that our clergy have been receiving an education far superior to that of any other class of men in this country—a more highly scientific education than our civil engineers, than the naval officers of the day, or the medical men of this country, and a more exact scientific education than even the scientific corps belonging to the British army. (Hear, hear.) These are open and notorious facts; and it is absurd for men to come forward and say that, with such an education as this, the clergy are not capable of discussing scientific questions, but that we are all trammelled and bound up by the Thirty-nine Articles. This is what Professor Huxley has virtually said. I do not know whether he intended to say so or not; but, I ask, what else could he have meant? The inference clearly is, that there are certain things which, in our own minds, we are obliged to admit,—which we cannot fail to admit,—but which we do not admit, because, for the sake of our daily bread, we have to subscribe to the Thirty-nine Articles. I ventured to call Professor Huxley's attention to this; and I must say that I do not think that those who were present at the meeting at Sion College considered the discussion to have been very fairly conducted. I wished to speak on the subject of the supposed discrepancy between the opinions held by men of science and by the clergy, and stated that the matter was not to be narrowed to the mere question of chronology. I said that none of the clergy ever maintained the infallibility of Archbishop Usher's chronology. I stated that, we were told, in offensive terms elsewhere, of a far greater discrepancy, namely, that there were persons who were called "Adamites, pure and simple," who believed that 6,000 years ago God created Adam, and that out of his rib He took Eve; that the whole of his race were subsequently destroyed, with the exception of eight, who were saved in the Ark, and that that was called the "Adamitic theory, pure and simple;" that nine-tenths of the public were taught this and believed it,

and that Professor Huxley, in a paper in the *Fortnightly Review*, with his own name attached to it, had said, "Now, I do not believe this, and I know no scientific person or well-instructed person who does." When I ventured to call Professor Huxley's attention to this, Mr. Rogers, the President, told me that I had no right to import into the discussion—opened, let it be remembered, by Professor Huxley, and the subject of which, mind you, was the discrepancy existing between scientific and theological opinion—anything that Professor Huxley might have said or written elsewhere. I wished to know whether that was a really scientific opinion or not, and I was told that I had no right to import into the discussion anything that had been previously said or written by Professor Huxley. I wanted to fix Professor Huxley to the point where there was a real difference of opinion; for I think that this will be held to be a real divergence of opinion, if such an opinion be held by scientific men. But I hold very strongly—and I believe that here Dr. Gladstone will agree with me—that Professor Huxley has libelled the scientific men of this country in saying that no scientific or well-instructed person with whom he was acquainted believed in the Divine creation of Adam and Eve. I ventured to point out how far that divergence went—that it went further than denying the creation of Adam and Eve; and from the paper I have just referred to, I took a quotation which Professor Huxley did not deny was his writing. He told us—and I regard the manner in which he answered me as an insult to the clergy, the way in which he refused to answer me—that he should have thought it insulting to have imported anything which might be annoying to the clergy into remarks made by him as their invited guest. That would seem to imply that he did not consider us capable of entering upon the subject. I have said thus much because I wished to explain what it was that gave rise to what has been alluded to as the personal feeling shown in Mr. Reddie's paper. But, I ask, why does Professor Huxley deny the special creation of Adam and Eve? From the same paper, in which he talks of "what men hold to be a holy and divine truth," and in which he libels the clergy under the name of the "Adamitic genus, pure and simple," I get from Professor Huxley an admission that there is no scientific objection to the unity of the human race. In his own science, that of comparative anatomy, I should be very willing to accept Professor Huxley as an authority; but when he goes beyond that, I want to know the grounds upon which he makes his assertions, in order that I may get at what he means. As I have just said, I get from him an admission. It was held a few years ago by Lord Kames, and after him the dogma may be said to have been adopted by all the infidel schools, that you could not maintain the unity of the human race, because it was impossible to get a black man out of a white man. Professor Huxley, however, seems to have no such difficulty. In fact, it would seem that he would find little difficulty in believing it possible to get a black man out of a monkey! He holds that from comparative anatomy you have overwhelming evidence of the unity of the human race. But he objects to the proposition that the human race must have come from several centres, because that

would multiply the miracle of Creation ; and he refuses to admit the creation of Adam and Eve, because in doing so he would be admitting a miracle. To have created Adam and Eve would have been a miracle, and according to him, a miracle is not to be admitted. In Paley's "Evidences of Christianity" allusion is made to Hume's objection to miracles, and Hume's objection was that it was contrary to all experience that miracles should be true ; but that it was not contrary to experience that human testimony should be false, and, therefore, no amount of testimony could support miracles. But after a time the sophistry of this was discovered. It was found that the major premiss involved the whole question, because the whole question was that miracles were contrary to experience. This sophism having worked its way for a time, was taken up by the Rationalists of Germany, and by them converted into a form which has become very popular among some who make a noise in the world as scientific men. They, in order to eliminate the miraculous records from the Bible, give a definition of a miracle. Now, I must say that theologians are somewhat chary of giving a definition of a miracle, just in the same way as physiologists are chary of giving a definition of life. However, these men defined a miracle as something contrary to law, or an interference with law, and they asserted that no interference with law could take place. Therefore, when they set to work with their system of destructive criticism, they argued that everything which appeared to be miraculous was to be eliminated from the Bible. We now find certain men in our own scientific world who have become suddenly enamoured of this definition of a miracle, and they admit that creation must have been miraculous. There we are at one with them. But they go further, and they say, creation being miraculous, there could be no creation. Professor Huxley is an advocate for this theory, because he has a scientific objection to the idea of creation, on the ground of its unphilosophical character ; and he says it is unphilosophical to admit the creation of man. He states that the progress of science pushes the origin of things further and further back. There are others who have gone further back than Professor Huxley. He does not publicly state more than that to multiply the centres of the creation of man would be to multiply miracles. But we go further. Professor Baden Powell, combining with the representatives of the rational theology of Germany, and with those who were supposed to be men of advanced scientific opinion, no sooner saw Mr. Darwin's book, which, it should be understood, was only put forward as a matter of hypothesis, admitted not to be proved, and to be unprovable at present, and based upon negative evidence—no sooner did he see this book, than, in his celebrated essay in the *Essays and Reviews*, he went the length of denying creation at all, and proceeded on the eternity and self-evolving powers of matter. Now, that I maintain to be a denial, not only of the theological proposition, but of the principle which lies at the bottom of all great scientific discoveries ; namely, that all the visible works of creation have impressed indelibly upon them the evidence that they came from the hands of an intelligent Being, and that they are the production of a Creator. There is an objection to the use of the word Atheism ; and I do

not call the man who holds such opinions as those I have referred to an Atheist, but I do characterize opinions which would altogether exclude the Creator from the world, and would not only do that, but would exclude the power of an Omnipotent Being ruling and governing by His Will the visible things of the world—I do say that this Epicurean system, this system of no creation, this system of eternity of matter, must be characterized as an atheistical system, a system which has the effect of driving God out of the visible things of creation. (Cheers.) How would these men, while excluding God from the visible things of creation, admit God ruling and governing the moral being of man? They tell the clergy that that is what they have to deal with, and that we ought to leave to them, the physical philosophers, the questions relating to what they refer to as self-creating, self-evolving, self-educing matter. To answer them scientifically, I would ask those who hold such opinions whether they can tell me what is matter and what is force, and what is the distinction between matter and force? If they attempted to answer such questions and entered a little more deeply into the great problems of science, they would soon feel that ignorance which was expressed in no mere platitude by Sir Isaac Newton when he admitted that after all he had done, he, a physical philosopher, who, perhaps, saw more clearly and knew more of the constitution of matter than any man who has succeeded him, had simply been gathering a few pebbles from the sea-shore, while the great ocean of truth lay undiscovered before him. (Hear, hear.) That is the position which, I think, men of science ought to take up with regard to revelation. It would be much better if men of science would meet us as Professor Morris has done. But we say,—“Wait awhile.” We know not whether the chronology of the Bible be 6,000 years or 10,000; but we know that almost before the science of geology was thought or dreamt of by scientific Europe, theologians had a difficulty with regard to the interpretation of the first chapter of Genesis—as to whether the six days were days of four-and-twenty hours only, or whether the days referred to periods of thousands or even of millions of years. They knew not, and did not profess to know, the exact meaning of the term. It was the men of science who afterwards set the example, which some theologians have followed, and who thought that they could bring the Bible to support the theory of successive creations, a theory which Sir Charles Lyell now abandons as not supported by the facts of science. I may say that I agree with Professor Morris in a great deal that he has told us; but he knows that the subject of geology is as difficult a subject as a man can study. I myself do not pretend to be a geologist; but Professor Morris is aware how hard I have been at work for many years trying to understand the very alphabet of the science, and yet all I have learned is how little we really do know upon the subject. He well knows that geology must meet demonstrative and mathematical science, and how imperfectly geologists, in general, are acquainted with the very foundation of their science. If this be the case with regard to the alphabet, what will be our difficulty when we come to spell our letters out and to interpret the hieroglyphics of the science, for hieroglyphics they certainly

are. We have had no end of theories as to the formation of the earth. We had one theory that it was formed by fusion—that it was the result of a fiery mist condensing itself into Plutonic rocks by slow cooling. That theory has, however, been abandoned. In the growth of science, what were formerly believed to be the oldest rocks are now ascertained to be some of the most modern. Even granite is at the present moment, in some quarters, doubted to have been of igneous formation, but is considered to have been of aqueous formation. We have gone back from the Huttonian to the Wernerian theory; and at the same time Sir Charles Lyell tells us, that while the evidences obtained by geologists are throwing back the antiquity of man further and further; that while the old idea we had that all the different strata indicated periods of the earth's history when there was a remarkable uniformity totally unlike the varied fauna we see around us now—a layer of one formation, then another layer, and then an island lifted up and suddenly, or, perhaps gradually, deposited again,—that while this theory of gradual upheavals and depressions is still held, it is possible that we are getting another swing of the pendulum of geological opinion, and that in a few years all this will be abandoned. Sir Charles Lyell goes on to show that there has been a certain progress of the science, and Professor Morris to-night has told us that mistakes have been made; that there are none of the great gaps which used to be supposed, and that that supposition arose from our ignorance. With all this, it is a question whether we are not going back very much to what may be called the vulgar opinion with regard to the six days' creation. I say, therefore, that when the subject is so beset with difficulties, men of science ought to display the greatest caution. There never was a period when scientific men differed so much; and when, as Lord Bacon has expressed it, men of science adopt hypotheses which are to be upheld at all risks, and only accept the facts that agree with these hypotheses, then the tendency is to put back the growth of science. We have Sir Charles Lyell himself admitting that the theory of successive creations held by him in common with the majority of geologists, caused him to shut his eyes to evidence brought before him, which he did not admit until he became a convert to Darwin's theory. It is this hypothetical dealing with science of which I complain, because I know that it keeps back the truth in most important matters, and renders thousands and myriads of observations useless, on account of those who made them having been the slaves of theory. Almost all the great measurements of crystals which we have in our text-books are not the real angles observed; but the angles, after they have been observed, have been changed and twisted and made to correspond with certain notions—first with regard to molecular formation, and then with regard to the proportion of axes. While cultivating all the facts of science, and holding out the right hand of fellowship to every man who devotes himself to its cultivation, we say be careful of your hypotheses—hold loosely by them. I know of no science which is so certain that men should hold strongly by its hypotheses. I think there are but two cases where there is a tolerably exact con-

formity of the laws of science as derived from fact and hypotheses ; namely, the theory of gravitation and the theory of light. Now let us take the theory of light. Newton propounded a certain emission theory, and so long as that was capable of explaining all the known facts of optical science, it was admitted to be a true hypothesis ; but when facts came out which could not be made to square with it, did scientific men then say—"No ; it is treason to go against Newton. Who are you that you should dispute with him ?" On the contrary, scientific men, when they got at a new fact, and found that it was likely to lead to a new series of facts, welcomed it, and then they obtained—what ? Why, another theory, including all the Newtonian theory, together with the new facts which could not be made to square with it. The majority of philosophers now hold strongly to the undulatory theory ; but, while this contains phenomena not in conformity with the Newtonian theory, there are some very awkward phenomena which cannot be brought under the undulatory theory, though no one has been enabled to devise a new theory differing from both, and yet including the new phenomena. Then with regard to gravitation, if some discrepancies between that theory and the motions of the planets and satellites should be brought to light, they would be admitted to upset that theory. The theory has been held loosely by many men of science, and when they found a fact that did not square with it, they ignored it. There is a feeling among some mathematicians that the theory of gravitation, which has hitherto been boasted as the greatest product of the human intellect, will have to be abandoned. (Hear, hear.) There is a great discrepancy between the calculated elements of the last discovered planet and its observations. But the discrepancies discovered in astronomical science have not become known to the general public, because there are so few cultivators of pure science. There is a great charm about an uncertain science like geology, where every man can make some pretty little theory of his own ; and in geology it is comparatively easy to make these theories ; but when you come to the hard formulæ of mathematical science and all the complications of differentials, and find it many years before you can understand its hieroglyphics, and know how few men can combine the actual observations of the places of planetary and other bodies with those assigned to them by science, then it is a very different matter. At Cambridge there are many men who can write out for you the lunar and the planetary theories, but how few are there of these men who can handle the telescope and measure the positions of these bodies. It is, perhaps, no breach of confidence to tell you that even so great a philosopher as Babbage has made this admission to me :—"I am inventing (he says) an analytical machine, which the world is not yet ready for, and which scientific men are not yet capable of appreciating." He has shown me his working drawings, contained in I do not know how many portfolios ; and he says, "I have made it a condition in my will that these things are not to be published until half a century after I am dead and gone, because science will not have arrived at a period when my work will be understood until half a century has elapsed." This machine has been devised

principally for the computation of the constants in the planetary and lunar theories. Mr. Babbage says that the method of calculation is too Herculean for the human brain ; and he is therefore inventing a machine for the purpose of doing that which no human mind can accomplish. (Hear, hear.) The *Saturday Review* made a little fun of me in reference to an expression I made use of at Sion College, as to the difference between pure and mixed science. I suppose that that *Saturday Reviewer* must have been present, and that his notion of mixed science was something like the notion one might have of mixed spices ! (Laughter.) If he had known anything at all about science, he would have found that the term “ mixed science ” was one for which the “ *Encyclopædia Metropolitana* ” is responsible. Well, then, I say that, if we have to admit the hypotheses of pure science with such extreme caution, how are we to deal with mixed science ? Professor Morris has referred to a work which was, no doubt, written well up to its day, but which is far behind the science of the present day—Cardinal Wiseman’s lectures on “ *The Connection between Science and Revelation*.” Most of the difficulties there mentioned have been swept away by the onward progress of science. There was, however, one point taken up by Cardinal Wiseman which I believe to be a very sound one, and that was, that in the infancy of most sciences they appeared to be opposed to Revelation, but that as they advanced, their apparent opposition to Revelation was removed. He showed that that opposition arose from imperfect hypotheses, and that when these were corrected, it was found that the sciences which in the beginning appeared to be most antagonistic to Revelation were those which most peculiarly illustrated and verified the truth of Revelation ; so that as science advanced it was its lot to come more in accordance with what we believed to be revealed truth. (Hear, hear.) And I think, whether in regard to geology, anthropology, or language, that those who observe the fleeting, changing hypotheses of these sciences, must know and feel that so far from their progress diverging further and further from Divine Revelation, there are the strongest symptoms (where those sciences are only fairly cultivated, and men will stick to their facts irrespective of hypotheses) of a tendency to throw more and more light on that which we believe to be Divine Revelation. (Hear, hear.)

The Meeting was then adjourned.

NOTE A. (*See* p. 304.)

PROFESSOR HUXLEY'S LECTURE AT SION COLLEGE.

IN order to complete the history of this controversy, I shall here append the correspondence on the subject, as it appeared in the newspapers at the time.

The following paragraph is extracted from the *Record* of the 25th of November, 1867 :—

We have already directed attention to the lectures introduced to Sion College under the auspices of its president, the Rev. W. Rogers. Mr. Reddie (of the Victoria Institute), who was present on the occasion of Professor Huxley's lecture on Thursday evening, writes to us :—

“Professor Huxley delivered an extempore discourse upon the divergence between the scientific and clerical mind, taking his text from the forty-first chapter of Genesis, relating to Joseph's promotion ‘to ride in Pharaoh's second chariot.’ The clergy were at first given to understand by the Professor that the pyramids stood upon mud, and, if so, that they would be very foolish to believe that the mud was put under the pyramids instead of that the pyramids were built over it ; and a good deal more like this was said, ‘which nobody can deny.’ Of course, as the pyramids could not have floated upon mud, and as they are actually built by the intelligent Egyptians upon solid rock, the argument was not sublime ; and perhaps I may say, without offence, that it was even almost superfluous. Many of Professor Huxley's arguments were equally simple ; and it was frankly stated at the meeting that, perhaps, ‘even the clergy’ would ‘admit nine-tenths of all he said.’ But the remaining tenth (which probably not one of them would at least as easily admit) was but vaguely advanced against the Bible chronology, —if, indeed, as Mr. Simcox Lea very pertinently observed, there was anything new advanced at all. Still, the matter cannot honestly be left in this vague condition ; and I enclose the copy of a letter I have just addressed to the President of Sion College, in hope of getting some more satisfactory discussion of what was insinuated, rather than argued or proved, at the very poor discussion last evening.”

The following is a copy of Mr. Reddie's letter to Mr. Rogers :—

“Bridge House, Hammersmith, W.

“Nov. 22, 1867.

“Rev. and dear Sir,—I beg leave to forward to you, as I promised last evening, for the library of Sion College, the first volume and Nos. 5 and 6 of the *Journal of the Transactions of the Victoria Institute, or Philosophical Society of Great Britain* ; which Society was established for the express purpose of investigating fully and fairly, and discussing, such questions as Professor Huxley treated of last evening,—not in an ephemeral, half-and-half way, as between hosts and guests, but as between men and men who cannot give, and do not ask, where they differ, for any intellectual quarter, on any plea of superiority or prejudice, on one side or the other ; and which Society, as you will observe, prints what is said on both sides, so that there may be no waste of time, or mistakes about meaning, or any giving up arguments from Nile mud or anything else, unless they are fairly refuted.

"Permit me again to thank you for allowing me to be present, and to speak the few words I did last evening. You would observe that Professor Huxley did not answer my question as to whether he was prepared to adhere to the notion that the Atlantic ooze is simply a 'deposit,' as he called it; nor did he tell us the supposed rate of its deposition, though he made that the sole criterion of the implied enormous time required for the chalk formations. I beg now to say, that, as Professor Huxley refused my challenge to set down his arguments in print, and to read them or allow them to be read and discussed, in the Victoria Institute, I shall at once write a reply to his discourse, which I shall be glad to read in Sion College within a week, if you will grant me this permission. But if not, then I shall print my reply, and take the liberty of distributing it among the members of Sion College, and also publish it, in order to go before the only 'tribunal' to which I last night ventured to summon Professor Huxley—namely, that of the intelligent and reading general public of this country.

"I have the honour to be, Rev. and dear Sir,

"Your faithful Servant,

"J. REDDIE,

"*Hon. Sec. Victoria Institute.*"

"The Rev. President ROGERS, Sion College, City."

In the *Record* of the 26th of November, 1867, the following paragraph appeared :—

Mr. Reddie informs us, that the Rev. W. Rogers having replied to his letter which appeared in last Monday's *Record*, to the effect that it is not in his power to offer the use of the hall of Sion College for the purpose of answering Professor Huxley—the meetings there being all arranged by the Court, and a scheme drawn out from which he knows they are not prepared to deviate—he (Mr. R.) has replied as follows :—

"Victoria Institute, 9, Conduit Street, W.

"Nov. 25, 1867.

"Rev. and dear Sir,—In answer to your letter of the 23rd (received to-day), the decision in which I regret, I can only say that I have already written my reply to Professor Huxley, and it must be delivered somewhere. It would not, in my opinion, be honest towards the Christian public to allow such things as were spoken by Professor Huxley 'to be done in a corner,' and not answered.

"I shall now consult the Council of this Society as to whether my reply may be read and discussed here; and you will observe from this evening's *Record* that I have made the matter *juris publici*.

"I have the honour to be, Rev. and dear Sir,

"Very faithfully yours,

"J. REDDIE,

"*Hon. Sec. Victoria Institute.*"

"To the Rev. President ROGERS, Sion College, City."

Mr. Reddie adds, that the Council of the Victoria Institute have decided to appropriate a Special Meeting before Christmas for discussing this subject, of which Meeting due notice will be given. A gentleman who was present at Sion College, and took *verbatim* notes of the learned Professor's remarks, has kindly offered to place his notes at Mr. Reddie's service.

NOTE B. (*See* p. 346.)

REMARKS ON THE DISCUSSION, IN REPLY.

BEFORE proceeding to notice the few issues arising upon the discussion of my Reply to Professor Huxley, which appear to require some explanation or answer, I beg leave first to be allowed to acknowledge the kind expressions of sympathy by the members of the Institute towards myself, under the sad circumstances which prevented my being present at the meetings in which my paper was read and discussed. I have also to thank the Rev. Dr. Thornton for kindly reading for me so long a paper, and for reading it—as I have been informed and as I expected—so very admirably.

As regards the length of the paper itself, I must observe that it was written in answer to a discourse which occupied a very long time in delivery; and being a “reply” to what was spoken elsewhere, it was necessarily lengthened by the statement of my opponent’s arguments in addition to my own. I may also point to the fact that a great number of distinct arguments required to be brought under discussion, each one of which might well have formed the subject of a separate paper; but I was in this obliged to follow Professor Huxley, in reply to whom I wrote.

I must further premise that it was too much forgotten by some who took part in the discussion, that my paper is only a reply, and that it was not written to advance or establish arguments or propositions of my own, but to refute those advanced and propounded as established scientific doctrine by Professor Huxley. The question raised by Mr. Greig, therefore, as to whether geology is or can ever be a science, was beyond the scope of the controversy. He certainly ventured upon a strong expression when he said, “Geology is not science, it is pure conjecture;” and I am not surprised that Dr. Gladstone and others should demur to it. But, strangely enough, the learned Doctor himself did his best to establish the merely conjectural character of geological chronology—the only deductions of geology then under consideration,—by himself rejecting “the hundreds of thousands of years which some of our friends speak about” (p. 362, *antè*), and *à fortiori*, therefore, rejecting Professor Huxley’s “millions of years;” and this he would surely not have dared to do, had these “millions” or “hundreds of thousands of years” been deductions of science instead of the “merest conjecture.”

But in what respect, let me ask, are Dr. Gladstone’s own views superior in character to those of Professor Huxley? Professor Huxley, at all events, thought each of his arguments cogent, and therefore that, taken altogether, they formed a cogent array of proof in favour of his conclusions. But Dr. Gladstone only advanced a series of arguments founded on controverted points, upon each of which he gave his own not very definite opinions; and then he added, “I do not say that any one of these arguments is conclusive

in itself, but I contend that combined they afford a very strong proof," &c. (p. 361, *antè*) ; as if several nothings could amount to something, or a series of inconclusive arguments could compose a science, or ever become a very strong proof !

It is quite in keeping with this conjectural kind of evidence that Dr. Gladstone believed that Professor Huxley was "to a considerable extent misunderstood" by me, *because* (the Doctor also believed) a gentleman was present in the Victoria Institute better acquainted with the subject who would be able to show that to be the case ! I suppose he referred to Professor Morris, or possibly to Mr. Row, who described one thing which he considered misquoted from Professor Huxley, as "abominable nonsense" ! But it seems that neither of these gentlemen heard Professor Huxley's address, or were really any better acquainted with it than Dr. Gladstone. All who did hear the Professor's address testified that I had *not* misunderstood him. And even Dr. Gladstone, I am glad to find, afterwards qualified this strange "argument from authority" by saying that he did not mean to state that I had misunderstood the main scope and purport of Professor Huxley's address, but merely some of its details. What these details might be we are not informed. But the two grand points which formed the scope and purport of Professor Huxley's address were discussed by Dr. Gladstone himself, and on both these points he differed entirely from Professor Huxley ! I have already alluded to one of them—the geological chronology of hundreds of thousands and millions of years—which Dr. Gladstone "does not go to the extent of believing." The other was "Professor Huxley's error" (p. 347, *antè*), that there is an opposition between science and religion, and which he went to Sion College expressly to declare, but which Dr. Gladstone says is only the teaching of "the infidel halls of London." I might say more with reference to Dr. Gladstone's other remarks on this point, but I prefer to refer to our *Journal of Transactions*, vol. i. pp. 142, 144, where it will be found that what he said has been already answered.

I must, however, agree with Dr. Gladstone as to false science being sometimes unfortunately preached from our pulpits. My quotations from the *Saturday Review* and from Mr. Warrington (pp. 305, 306) explain how this comes about ; and the foot-note on p. 36 of our first volume illustrates it still better. The gentleman there referred to, who boasted that he had "taught the same geology for fifty years," is a scientific clergyman. The concluding words of my paper are actually a warning against this, which Dr. Gladstone appears to have overlooked.

Professor Morris was very well answered by the Chairman, with reference to the Atlantic soundings ; and he afterwards appears to distrust his own statement as to any "soundings" fourteen feet deep in the chalk ! I doubt very much myself whether they penetrated the chalk-ooze to the extent of even four inches, in fetching up the specimens for microscopic investigation. On several points Professor Morris *believes* that I misunderstood Professor Huxley. But, if I did not, that only means that the two Professors are at issue on all such points just as other great geologists are known to be at issue—for

instance, Sir Roderick Murchison and Sir Charles Lyell—as regards successive creations. Professor Morris is mistaken in supposing that “Professor Huxley alluded to twenty-nine or thirty distinct formations” marked by “distinct organic remains.” (p. 356.) *I* alluded to there having been “once no less than twenty-nine supposed successions of life on this earth,” which was a view advanced by M. d’Orbigny, and entertained for some time by many as geological science. Professor Huxley said nothing about it, or of any modifications of it subsequently, but mentioned only “three successions—three revivals.” And why he spoke even of these, unless to suggest to his hearers the idea of three special creations, I do not understand; and yet it is notorious he does not believe in any new creations, or in such “revivals” or “successions” of distinct genera and species. He has distinctly said that those “appearances” of new genera “*may be the simple results of migration.*” (p. 331.) If, then, as would appear, Professor Morris *does* believe in “special creations,” he is at issue with Professor Huxley; and their diverse opinions cannot both be “science.” If the clergy unfortunately preach either view, they will be liable to be arraigned as “clearly unscientific” by the adverse party; just as they have been by Dr. Gladstone in this discussion, without his telling us, however, what scientific theories they had propounded from the pulpit to his dissatisfaction. Professor Morris introduces some of the stock arguments of geology bearing upon man’s antiquity, which appear to me anything but cogent. For instance:—There are no evidences of man’s remains—no fragment of a canoe wrecked among the coral reefs of the Carboniferous period;—and *therefore* (it is argued) man did not then exist! Now apply this to the Atlantic chalk ooze. Before Columbus crossed the Atlantic (and if other unknown navigators did not precede him) there could, of course, be no coins, copper kettles, or anchor-stocks, or any other specimens of man’s handiwork, dropt into the Atlantic and embedded in the ooze. And *therefore* (with equal want of cogency) it might be argued, that no men existed on the earth before Columbus, if the “evidence” depended upon the Atlantic chalk up to that date! And so there are no evidences of man’s remains “among the Saurian bones of the lias.” (p. 356.) *Therefore* (because Saurians lived in water and man on land,) man is proved not to be in existence anywhere, his remains not being found among Saurian bones! Then as regards Professor Morris’s argument as to the antiquity of man’s remains where they have been found in the valley of the Somme, I must refer to p. 174 of this volume of our *Journal of Transactions*, and what is there said as to these and the cognate “finds” in Auvergne.

But I must hasten on, to notice the remaining adverse criticisms—those of the Rev. Mr. Row. I think I shall most effectually answer him, by submissively accepting the position of one of his boys, whom he, as head master of a grammar-school, had made to write out the whole book the boy had purported to quote from, and quoted inaccurately. (p. 350.) I purported to quote Professor Huxley, not Herodotus, in the passage which I regret called forth Mr. Row’s very pungent remarks. I append the complete epitome of Professor Huxley’s address, as it was taken down and sent to me by a

gentleman I have never seen in my life. I think the Professor has been admirably reported, though in brief ; and I cannot see Mr. Row's difficulty in the passage referred to (p. 308), even as I quoted it, especially after the pains I took to show that Professor Huxley had not cited his author very accurately, and to give all the passages that I could myself find in the old Greek historian bearing on the point. I am rather amused that Mr. Row should seem a little put out because I did not set up distinctly a biblical chronology of "only 6,000 years," and so give him an opportunity to "beat me upon that issue"! I must say I prefer not to be beaten, and beg to refer him to p. 303 as to what I undertook to do ; but I may add that "only 6,000 years" may be perfectly true for anything that Professor Huxley proved to the contrary.

In conclusion, I have to thank the Chairman, Dr. Irons, and Capt. Fishbourne, who did hear Professor Huxley, for their kind defence of my paper and arguments. But I must say that I do not think Professor Huxley was guilty of reviling, nor that I (as Mr. Row expressed himself) did "revile again." (p. 363.) I spoke plainly in answer to very plain speaking ; but throughout the whole discussion, it appears to me that no one has used such strong language as Mr. Row himself. I had almost omitted to notice that, in alluding to Socrates, and to the Sophists as "the professors of *his* day," I did not mean "to connect them with *all* modern professors"! Besides "the Sophists," I do not know what other "professors" there were in the days of Socrates, "who went about teaching for profit their deleterious sophisms," as Plato tells us they did ; and I don't know how "they could have existed unless they had taken money for their teaching." Nor, in fact, did they know themselves ; and that is why they were so angry with Socrates, who denounced their teaching as both false and mercenary.

I must add, with reference to one part of our respected Chairman's remarks, that as I believe in neither the current planetary nor lunar theories, I am glad to hear that reasonable beings are about to leave "the computation of the constants" to Mr. Babbage's machine of the future, which he is "inventing for the purpose of doing what no human mind can accomplish!" (p. 370.) It may be my misfortune, but I confess I am quite unable to believe in this machine! Consequently, I am delighted to think that the planetary and lunar theories will most probably themselves be given up—I hope before Mr. Babbage's will is proved,—since it seems now to be acknowledged by some of our mathematicians that the theory of gravitation itself "will have to be abandoned." (p. 369.)

The following is the abstract of Professor Huxley's address, as it was published in the *Record* of 7th February, 1868, from the notes sent to me as stated on p. 304. Those who will carefully read it over, and then look back at my citations from it, will now be able to judge how very fully and fairly I quoted and represented the learned Professor's words and arguments :—

ADDRESS BY PROFESSOR HUXLEY TO A BODY OF THE
CLERGY AT SION COLLEGE.

Nov. 21, 1867.

IN coming here to-night at the request of your President, I beg it may be distinctly understood that for what I may say I alone am responsible. One of the things which strikes us in these times is the fact that there are two great leading sections of society, *i.e.*, philosophers and clergy, which occupy positions towards each other which are neither pleasing nor wise. These two portions of society at one time taught but one doctrine, although they represented different sides of that doctrine; but now it is not so—the views of each have become more and more divergent, although the fact remains that philosophy and theology are but different sides of one and the same thing.

You clergy, from a sort of conventional dishonesty of society, tend to widen that divergence. The mental atmosphere in which my friends, as scientific men, and the clergy live, are different—the two utterly distinct: the points of contact between the two very limited indeed. Intellectual communion there is none; each goes on, exists, and thinks in his own separate world.

This, to say the least of it, is lamentable; both are men of the same origin, the same interests, the same desire for truth. Why is it the divergence is so great? Your President has done me the honour of thinking that I, for the present at least, may be regarded as the representative of science and scientific research on this occasion, and I on this occasion accept that responsibility.

My business to-night is not to be the missionary, but the minister of science. I desire no converts, I seek to make no proselytes; I am not here to proselytize, and I desire most anxiously to abstain from anything that might jar upon the minds of those who hear me. The line which I purpose to take is simply this:—1st. What we men of science think; and 2nd. Why we think it. There are two ways by which the divergence between clerical and scientific opinion spoken of may be met. 1st. By the conversion of either side, which I fear I must pronounce to be hopeless. 2nd. By each side believing in the probity of the other, and trying to understand one another.

After this preface I shall make no further apology, but come direct to the point, and state clearly the conclusion we men of science arrive at by the deductions we are bound to make from existing facts. We cannot see our way out of these conclusions. Holding the principles we do, rationally and fairly, we cannot, in common sense and reason, draw back and give them up. We must go on to the legitimate consequence of those conclusions and of those principles.

You tell your congregations that the world was made six thousand years ago, in the period of six days—and further, that all living animals were made within that period, and on sundry of those days, and as made so have continued to the present time, making whatever deductions may be necessary for extinction of species and other changes since their original creation. Thus you hold and teach that men of science like myself are liable to pains and penalties, as men who are guilty of breaking or disputing great moral laws. I am bound to say I do not believe these statements you make and teach, and I am further bound to say that I do not, and I cannot call up to mind amongst men who are men of science and research, truthful men, one

who believes those things ; but, on the other hand, who do not believe the exact contrary.

And now let me state why we have these strong convictions. I desire to start from some facts and some data familiar to us both, both to you and to me. I have addressed various and varied audiences in my time, but never before a body of clergy like that before me. I will therefore deal with the subject in your own familiar method. I will take a text, and give you a scientific exegesis drawn from the text.

I will select a passage from the 41st chapter of Genesis, connected with the touching story familiar to us all—the history of Joseph and his brethren. We read in it “that Pharaoh took off his ring and put it upon Joseph’s hand, and put on him a gold chain, and made him to ride in the second chariot that he had.”—Now, I ask you to depict to yourselves that marvellous valley of the Nile where these events took place 1800 B.C. No doubt the passage is historical, that is to say, that the Pharaoh therein spoken of, who had at his disposition so great wealth, and who was master of the civilization of the world at that time, thought fit to elevate one of his slaves, invest him with symbols of authority, and made him to ride in the second chariot of the land—placed him in position, power, and authority next to himself. These things indicate great advances in civilization, and refinement, and luxury. Certain monuments of that era show horse-chariots sculptured upon them, as in Joseph’s time, when there must have been a great civilization. Before that, there existed a people highly civilized, but with whom are no traces of chariots or domestic horses. Thus we suppose a great interval elapsed. Now, when we examine the records of the past era, more than two thousand years before the Christian epoch, we find at Memphis, in the oldest pyramids, records indicating the high cultivation which existed then, as now, by the overflow of the Nile, and the fertility and produce consequent upon that. These monuments, built on the site of the great valley of the Nile, fertilized then, as now, by the deposits left by that overflow of the mud which became the source and cause of the land’s fertility and produce—these monuments evidently existed *after* this great deposit of mud upon which they stand ; and what is this Egyptian mud ? Herodotus asked this question five centuries before the Christian era. He said, this Nile valley, lying between great ridges of rocks, and becoming a huge receptacle for never-ceasing deposits of fertilizing mud,—this Nile valley, says Herodotus, was once a great arm of the sea, filled up in the process of time by mud brought down by the Nile. This great Nile valley, 1,200 miles long, filled up by mud forced down the Nile. And unless you are prepared to deny this condition of things, that in the time of Joseph and long before this Nile valley must have been essentially what it is now, ask yourselves what period of time this process of filling up this huge arm of the sea must have taken.

Various estimates have been made as to the quantity of mud which is brought down year by year. I will rather understate than overstate the results. The general estimate of the process of filling gives five inches in a century. This, no doubt, is a correct estimate, but let us take the quantity to be twelve inches, or one foot every century, so that there may be no room for cavil. Borings were made in the Nile valley for this purpose, and it was found that in the valley of the Nile we could bore to seventy feet through Nile mud. Seventy feet, at one foot for every one hundred years, gives at once seven thousand years, a longer period than has elapsed, according to the received opinion, since the creation of the world.

I come to the next point. The valley of the Nile, as stated by Herodotus, is enclosed by high rocky mountains, a long narrow valley, with great cliffs on each side. Now, in these rocks or cliffs Herodotus and Strabo both noted organic bodies, called by them, from their resemblance to a piece of

coin, nummulites, and this name is retained by us. Now these rocks were then, and are now, full of these nummulite formations. They can be traced from the land of Egypt as far as India and China, and westward as far as the south of Britain, covering full 98° of longitude, east and west. When we examine the structure of these creatures, we find the shell very exquisitely chambered, and the organization very elaborate and complex. They were once, without the shadow of a doubt, living creatures. The diagrams here presented show their organization and formation under very powerful magnifying power. Under what condition were they alive, and under what are similar creatures alive now?—for though there is nothing now exactly identical with these nummulites, yet there are many species like them that exist now, no doubt under the same conditions as they did. There is no doubt they were sea-living things, and approach closely to all those organizations which live in the sea; therefore there must be more than a probability that they were once marine inhabitants; and if so, it is reasonable to suppose that other marine remains would be also found in the same rocks. What are the facts? Dacier has described four hundred; he gives four hundred descriptions all identical with marine creatures now existing.

Put the case to yourselves. Suppose in walking on the paths in St. James's and Regent's Park, you see on the ground certain little shells—such, for instance, as cockle shells. You would say at once the walks were gravelled with sea-gravel, because the marine remains you notice in the sand are the fossils of creatures that lived only in the sea, and therefore, beyond dispute, the gravel with which they are associated must have come from the sea. This is very simple, plain, and perfectly valid. Now apply this plain common-sense reasoning to the point in question: if these betray marine origin, so these four hundred descriptions of organizations of sea habitants afford evidence that this “nummulitic” limestone has been deposited from the bottom of the sea; therefore that this deposit was formed at the bottom of the sea; therefore before the Nile valley was formed, and raised by subterranean forces, the land of Egypt was down at the bottom of the sea, and existed not only seven thousand years, but all that epoch which by slow accumulation would have furnished such a mass of “nummulitic” rock, spreading as it does from Hampshire to China. How many years? Thirty thousand? More; the time which this process occupied was an enormous period. And even this is but as it were an incident in the history of this earth,—no more than the shadow of a cloud passing over the history of the world. These rocks, traceable in England at Bagshot and elsewhere, contain the same organizations as in the valley of the Nile. Now what is the simple deduction? Cut through this country towards the west, towards Swanage, and there you find vast chalk ridges, and these other beds [pointing to a diagram] all rest on huge masses of chalk. Resting on the bosom of that are these rocks, containing the “nummulites,” and others, where we find great diversities of organizations—forms of crocodile life, nautila, &c., like those now found in tropical seas,—the great majority different from those which now exist, but all of the same genus—sea-inhabiting creatures.

Now, what is this chalk, which lies below the “nummulitic” formation? Is it there without meaning,—is it nothing but a mere nondescript substance, of which we can give no account? Here is a piece,—I put it under a microscope,—and what do I see? That it is one mass of organisms, or fragments of separate organizations. Now, what is the origin of all this? We can give a clear answer. Not many years ago, when the world became impatient for telegraphy, the sea-bottom of the great Atlantic Ocean was surveyed, and the result of that survey was very remarkable. Products from depths of 1,500 feet were sent to me; the stuff which was brought up resembled grey mud, or chalk when dry; and when inspected under the microscope, it

was found to be made up by 95 per cent. of the same organisms as chalk. Also in this deposit were curious things, which, when carefully examined, were found of the same organic structure.

Now to the point. The two organisms are not merely similar, but identical; both composed of animal organisms—the chalk and sea deposits; both existing under the same conditions. We might just as well say a man can live in oxygen gas, as that these low organisms, which left their exuviae at the bottom of the sea, could exist apart from the conditions under which they exist now. No man of science makes assertions like these without cross-confirmations. Chalk contains thousands of different organizations. There are the remains of four-footed animals, birds, reptiles, &c.—creatures which are not now living. This chalk extends over an immense area, far greater than the “nummulitic” deposit. Now the question is very simple. If those who have gone into these things find a flaw in each other’s data or conclusions, they are exceedingly ready to do so. A million years could not have produced this deposit of 1,100 feet thick—whether less or more it makes no difference; but it is clear this world was not made six thousand years ago!

But we must also admit that all the animals now living were not created as narrated in the Mosaic history of the creation. The animals which lived in the nummulitic period were not the same as those which now live—they were not the beasts, nor the fowls, nor the creeping things we now find, which existed in the Chalk period. There is positive proof of three successions—three revivals of inhabitants—of this world. Do we not see then the unknown previous duration of this earth? Apply the same principles to matters connected with daily life, and you are convinced at once of the correctness of our deductions.

If you take the only line of argument open to you, to help you out of the difficulty—if you deny our right to reason thus legitimately, remember it is, and it must be, suicidal to the other side. These views, of which, as the minister of science, I am the exponent to-night, are held by men who are as Christian in motive and practice as you. These doctrines are held by men who think deeply, and who have children to come after them, whom they desire to instruct wisely. They are held by the best of men. They are held out of no wantonness, or irreverence, or eccentricity. They are held by men who seek to discover to themselves, and to present to others, scientific truth. I ask you to remember this, to consider this, and then I ask you to judge us.

ORDINARY MEETING, JUNE 3, 1867.

CAPTAIN E. G. FISHBOURNE, R.N., C.B., IN THE CHAIR.

The Minutes of the last Meeting were read and confirmed, after which the following paper was read by the author:—

*ON THE GEOMETRICAL ISOMORPHISM OF CRYSTALS
AND THE DERIVATION OF ALL OTHER FORMS
FROM THOSE OF THE CUBICAL SYSTEM.* By
REV. WALTER MITCHELL, M.A.

1. WHEN elementary substances, or their chemical combinations, pass from a state of vapour; or from a fluid condition into that of a solid; or if they are deposited by evaporation from a fluid holding them in solution, there is a tendency of their particles to arrange themselves according to certain laws of symmetry.

2. Thus solids more or less symmetrical, and with few exceptions bounded by smooth, plane, or flat surfaces, are produced. Such solids are called *crystals*, and their plane surfaces are termed *faces*.

3. Some crystals are remarkable for perfect symmetry of form. Among these may be found solids formed with mathematical accuracy, whose geometrical properties had fascinated the ancient geometers ages before they were known to exist in the productions of nature. Others are exceedingly complex, being formed by the combination of faces parallel to those belonging to several simpler forms; the relative positions of these simpler forms to each other being regulated by certain mathematical laws.

4. The more complex forms being reduced to the combination of the simplest from which they can be derived, it is found that all the simpler forms can be grouped together in six distinct classes or systems.

5. The crystals of any one substance may generally be reduced to forms belonging to one system; but there seems to be no limit to the number of combinations of different species of these forms which may take place in any individual crystal.

6. To the rule that all the crystals of a particular substance should have their faces parallel to those of the forms of one system, there are numerous exceptions.

7. The following are the six systems:—

- 1st. The *Cubical*; called also the tesseral, tessular, octahedral, regular, isometric, and monometric.
- 2nd. The *Pyramidal*; called also the tetragonal, square prismatic, quadratic, monodimetric, dimetric, four-membered, viergliedrig, and the two-and-one axial.
- 3rd. The *Rhombohedral*; called also the hexagonal, monotrimetrical, sechsgliedrig, and the three-and-one axial.
- 4th. The *Prismatic*; called also the rhombic, trimetric, binary, unisometric, orthotype, orthorhombic, zweigliedrig, and one-and-one axial.
- 5th. The *Oblique*; called also the monoclinohedric, hemiprismatic, hemiorthotype, clinorhombic, hemihedric-rhombic, augitic, zwei-und-eingliedrig, and the two-and-one-membered.
- 6th. The *Anorthic*; called also the doubly oblique, triclinic, triclinohedric, anorthotype, clinorhomboidal, tetarto-prismatic, tetarto-rhombic, eingliedrig, and the one-and-one-membered.

CUBICAL SYSTEM.

8. The forms of the cubical system possess the highest possible degree of symmetry when compared with those of the other systems. They are divided into two groups,—the *holohedral*, or perfectly symmetrical, and the *hemihedral*, or half-symmetrical; the latter being derived from the former by being parallel to, or possessing only half their faces, grouped together after certain laws.

9. The *holohedral*, or perfectly symmetrical forms, are seven in number, and are shown on Plate I. Of these, three—the cube (fig. 1), the octahedron (fig. 7), and the rhombic dodecahedron (fig. 8), are invariable forms, each having but one species, and each the same invariable angles, either of their faces or inclination of their faces.

The remaining four forms are not invariable, and there are an infinite variety of species, each differing from the other in the angles of their faces and their inclinations to each other.

The half-symmetrical, or hemihedral forms, are represented in figs. 15, 17, 19, 21, 23, and 25, Plate III.

Holohedral forms, cubical system.

10. The CUBE (fig. 1, Plate I.) is bounded by six equal faces, each face, such as $O_1O_5O_8O_4$, being a perfect square;

it has therefore eight solid angles, $O_1, O_2, \&c., O_8$, each angle being formed by the union of three planes; and twelve equal edges, such as $O_1O_2, O_2O_3, \&c.$ The inclination of any face to another is measured by the angle contained between two perpendiculars drawn from any point in the edge made by the intersection of the two faces, each on one of the adjacent faces. In the cube this inclination of two adjacent faces is 90° . The facial angles, or the angles between two edges of a face, such as $O_4O_1O_5$, are always 90° .

11. The OCTAHEDRON (fig. 7, Plate I.) is bounded by eight equal faces, each face, such as $C_1C_2C_3$, shown on a plane surface (fig. 33, Plate IV.), being an equilateral triangle. It has six solid angles, $C_1, C_2, \&c., C_6$, each formed by the union of four planes, and twelve equal edges; the inclination of adjacent faces is an angle of $109^\circ 28'$, and the facial angle, such as $C_1C_2C_3$, is 60° .

12. The RHOMBIC DODECAHEDRON (fig. 5, Plate I.) is bounded by twelve equal faces; each face, such as $o_1C_2o_5C_3$ (fig. 30, Plate IV.), is a geometrical rhomb bounded by four equal lines, o_1C_2 being parallel to o_5C_3 , and o_1C_3 to o_5C_2 . The greater angles of the rhomb $C_2o_1C_3$ and $C_3o_5C_2$ being $109^\circ 28'$, and the lesser, $o_1C_2o_5$ and $o_1C_3o_5$, $70^\circ 32'$. It has twenty-four equal edges, such as $C_1o_1, C_1o_2, \&c., o_8$, eight solid angles, $o_1, o_2, \&c., o_8$, formed by the union of three planes, and six solid angles, $C_1, C_2, \&c., C_6$, formed by the union of four planes. The inclination of adjacent faces is 120° . This form is called by some German writers the *granatoëdron*, as being a characteristic form of the *garnet*.

13. These three forms, the *cube*, *octahedron*, and *rhombic dodecahedron*, are called invariable forms, as, though differing in size, they always have similar faces and angles; that of the cube being a square, that of the octahedron an equilateral triangle, and that of the rhombic dodecahedron a rhomb whose larger angle is $109^\circ 28'$.

14. The four other forms (figs. 2, 3, 4, and 6, Plate I.) are called variable, each presenting an infinite variety of species, differing from each other in their angles of inclination and those of their faces.

15. The THREE-FACED OCTAHEDRON (fig. 6, Plate I.) is bounded by 24 equal faces, each being an isosceles triangle, $o_1C_2C_3$ (fig. 32, Plate IV.). These faces are so grouped together as to form a solid having eight solid angles, formed by the union of three planes, $o_1, o_2, o_3, \&c., o_8$ (fig. 6); the plane angles being the largest of the isosceles triangles; and six solid angles, $C_1, C_2, \&c., C_6$, each formed by the union of eight of the equal angles of the isosceles triangles.

There are 12 longer edges, such as C_1C_2 , C_1C_3 , &c., and 24 shorter, such as o_1C_1 , o_1C_2 , &c. The 12 longer edges are the edges of an octahedron. It may be formed by placing on every face of the octahedron a three-faced pyramid on a equilateral triangular base. The angles of these isosceles triangles differ in different species of the three-faced octahedron, within certain limits to be described hereafter.

The synonyms for this form are the *pyramidal octahedron*, *triakisoctahedron*, *trioctahedron*, and *galenoid*.

16. The FOUR-FACED CUBE (fig. 2, Plate I.) is bounded like the last by 24 equal faces, each being an isosceles triangle, such as $C_1o_1o_4$ (fig. 34, Plate IV.), but grouped so together as to form a solid having six solid angles, C_1 , C_2 , &c., C_6 (fig. 2), each formed by the union of four of the largest angles of the isosceles triangles, and eight solid angles, o_1 , o_2 , &c., o_8 (fig. 2), formed by the union of six of the equal angles of the isosceles triangles. This form has 24 shorter edges, such as C_1o_1 , C_1o_2 , &c., and 12 longer ones, such as o_1o_4 , o_1o_5 , &c. The 12 longer edges are those of a cube.

It may be formed by placing on every face of the cube a four-faced pyramid on a square base.

The angles of the isosceles triangles differ for each particular species of the *four-faced cube*.

Synonyms.—*Pyramidal cube*, *hexatetrahedron*, *tetrakis-hexahedron*, and *fluoride*.

17. The TWENTY-FOUR-FACED TRAPEZOHEDRON (fig. 4, Plate I.) is bounded by 24 equal faces, each face being a deltoid or trapezium, $C_1d_1o_1d_2$ (fig. 39, Plate IV.); that is, a four-faced figure having two longer equal sides, C_1d_1 and C_1d_2 , and two shorter equal sides, o_1d_2 , o_1d_1 . These 24 equal trapeziums are so grouped together as to form a solid having six solid angles, C_1 , C_2 , &c., C_6 , formed by the union of the plane angles of four trapeziums, equal to $d_1C_1d_2$; eight solid angles, o_1 , o_2 , &c., o_8 , formed by the union of the plane angles of three trapeziums, equal to $d_1o_1d_2$; and 12 solid angles, d_1 , d_2 , &c., d_{12} , formed by the union of the plane angles of four trapeziums, equal to $C_1d_1o_1$. This form has 24 equal longer edges, such as C_1d_1 , C_1d_2 , and 24 shorter edges, such as o_1d_1 , o_1d_2 , &c. The angles of the deltoids or trapeziums differ for each particular species of the *twenty-four-faced trapezium*.

Synonyms.—*Icositessarahedron*, *icositetrahedron*, *trapezohedron*, and *leucitoid*.

18. The SIX-FACED OCTAHEDRON (fig. 3, Plate I.) is bounded by 48 equal faces, each face being a scalene triangle, $C_1o_1d_2$ (fig. 36, Plate IV.). These 24 triangular faces are so grouped together as to form a solid having six solid angles, C_1 , C_2 , &c.,

C_6 , each formed by the union of eight equal plane angles at the points C_1, C_2 , &c.; eight solid angles, formed by the union of six equal plane angles at the points o_1, o_2 , &c., o_8 ; and 12 solid angles, formed by the union of four plane angles at the points d_1, d_2 , &c., d_{12} .

This form has 24 edges, each equal to the edge C_1d_1 , 24 each equal to the edge C_1o_1 , and 24 each equal to o_1d_1 .

The angles of the triangular faces of this form differ for each particular species of the *six-faced octahedron*.

Synonyms.—*Hexakis-octahedron*, *hexoctahedron*, *tetrakonta-oktaëdron*, *pyramidal granatohedron*, *triagonal polyhedron*, and *adamantoid*.

19. These seven forms, grouped together on Plate I., have this relation in nature, that any substance forming crystals of any one of these forms may, and does sometimes, form crystals of any one of the other forms, or parallel to their faces. But when these forms are combined on any one crystal, as in fig. 29*, Plate IV.*, the forms to which the faces are parallel, except in the case of what are called twin crystals, always have a certain fixed position with regard to each other. These forms have not only this natural relationship to each other, but they have also certain geometrical relations, which we shall proceed to describe.

20. Looking at Plate I., the forms present no relationship to each other. Plate II. shows them connected together by beautiful geometrical laws.

21. In Plate II. we see that each of the six other forms can every one of them be inscribed, as geometers term it, in the cube.

Fig. 8, Plate II., shows the *cube* having each of its faces divided into eight equal triangles, by joining the opposite angles of each square by two diagonals, such as O_1O_8, O_4O_5 , meeting in C_2 , the centre of the face, and by two other lines, such as D_1D_9, D_8D_5 , also meeting in C_1 , and joining the centres D_1, D_9 of the edges O_1O_4, O_5O_8 , and D_5, D_8 , the centres of the edges O_1O_5 and O_4O_8 .

Fig. 9, Plate II., shows the *Four-faced cube* inscribed in the cube, and we see that the six solid angles of the twenty-four faced cube, C_1, C_2 , &c., C_6 touch the six centres of the six faces of the circumscribing cube.

Fig. 10. The *Six-faced octahedron* inscribed in the cube, six of its solid angles, C_1, C_2 , &c., C_6 , touching the centres of the six faces of the circumscribing cube.

Fig. 11. The *Twenty-four-faced trapezohedron* inscribed in the cube, six of its solid angles, C_1, C_2 , &c., C_6 , touching the centres of the six faces of the circumscribing cube.

Fig. 12. The *Rhombic dodecahedron* inscribed in the cube,

six of its solid angles, $C_1, C_2, \&c., C_6$, touching the centres of the six faces of the circumscribing cube.

Fig. 13. The *Three-faced octahedron* inscribed in the cube, six of its solid angles, $C_1, C_2, \&c., C_6$, touching the centres of the six faces of the circumscribing cube.

Fig. 14. The *Octahedron* inscribed in the cube, its six solid angles $C_1, C_2, \&c., C_6$, touching the centres of the six faces of the circumscribing cube.

CUBICAL AXES.

22. The lines formed by joining the opposite centres of the faces of the cube C_1C_6, C_5C_3 , and C_2C_4 (fig. 27, Plate IV.), are called the *cubical axes* of the cube. These three lines are equal to each other, and are perpendicular each to two opposite faces of the cube; they intersect in A , the centre of the cube. In fig. 27 two other sets of axes are shown, four O_1O_7, O_2O_8, O_3O_5 , and O_4O_6 , joining the opposite solid angles $O_1, O_2, \&c., O_6$, of the cube; six others, $D_1D_{11}, D_2D_{12}, D_3D_9, \&c., D_8D_6$, joining the opposite centres $D_1, D_2, \&c., D_{12}$ of the edges of the cube; both sets of axes passing through A , the centre of the cube. The four axes $O_1O_7, \&c., O_4O_8$, fig. 27, Plate IV., are evidently the four diagonals of the cube, and are represented fig. 9, fig. 10, &c., to fig. 14, Plate II., by lines marked thus — — —. The line D_1D_{11} , fig. 27, is parallel and equal to a line drawn from O_1 to O_6 , and is therefore equal to a diagonal of one of the faces of the cube. The 12 axes $D_1D_{11}, D_2D_{12}, \&c., D_6D_8$, are therefore each equal to a diagonal of the face of the cube. These lines are thus represented — — — —, fig. 9, fig. 10 to fig. 14, Plate II.

OCTAHEDRAL AXES.

23. If the equilateral triangle $C_1C_2C_3$, representing one of the faces of the octahedron (fig. 33, Plate IV.), has its three sides bisected by d_1, d_2, d_3 , and C_1d_3, C_2d_2 , and C_3d_1 be drawn meeting each other in the point o_1 , this point o_1 will represent the centre of gravity of the triangle $C_1C_2C_3$, and any of the shorter lines do will be a third of the longer one, Cd . The octahedron inscribed in the cube fig. 14, Plate II., has all its edges bisected by the points $d_1, d_2, \&c., d_{12}$, and each equilateral triangle divided into six triangles by lines Cd meeting in $o_1, o_2, \&c., o_8$, the centres of the eight faces of the octahedron.

It will be seen in fig. 14 that the six axes, such as D_2D_{12} ,

pass through two opposite bisections, d_2, d_{12} , of the opposite edges C_1C_3 and C_5C_6 of the octahedron.

The four axes, such as O_1O_7 , pass through the centres o_1, o_7 of the opposite and parallel faces, $C_1C_2C_3$ and $C_6C_5C_4$ of the octahedron, and are perpendicular to both of them.

Owing to this property, the four axes O_1O_7 , &c., O_4O_6 , are called the *octahedral* axes of the cube.

24. This property may be demonstrated as follows:—

Describe a square (fig. 27*, Plate IV.*), $AC_1D_1C_2$, having each of its sides $= O_1D_5$ (fig. 27, Plate IV.).

$AC_1D_1C_2$ is evidently a fourth of the square $O_1O_5O_8O_4$, forming a face of the cube (fig. 27, Plate IV.).

Draw the diagonals of the square C_1C_2 , and AD_1 , meeting in the point d_1 . C_1C_2 bisected in d_1 will represent on a plane surface in (fig. 27*, Plate IV.*) the edge of the octahedron $C_1d_1C_2$ seen in perspective in (fig. 14, Plate II.).

Produce D_1C_1 and C_2A (fig. 27*) to O_1 and D_5 , making C_1O_1 and AD_5 each $= AD$, a diagonal of the square $D_1C_1AC_2$.

Join O_1D_5 , make $Ad_5 = Ad_1$. Join C_1d_5 and AO_1 , meeting in o_1 .

Then $C_1o_1d_5$ and AO_1O_1 (fig. 27*) represent on a plane surface the lines similarly shown in perspective in (fig. 14, Plate II.)

25. To facilitate calculation we shall choose one of the sides of the square $C_1AC_2D_1$ as our unit.

$$\text{Then } AD_1 = \sqrt{2} \text{ and } Ad_1 = Ad_5 = \frac{\sqrt{2}}{2} = \frac{1}{\sqrt{2}}$$

$$\text{By plane trigonometry } \tan Ad_5C_1 = \frac{AC_1}{AD_5} = \frac{1}{\frac{1}{\sqrt{2}}} = \sqrt{2}.$$

And angle $Ad_5C_1 = 54^\circ 44' 8''$.

Now (fig. 14, Plate II.) the lines C_1d_5 and C_6d_5 are both by construction perpendicular to the edge C_2C_3 of the octahedron of two adjacent faces at the point d_5 .

The angle $C_1d_5C_6$ therefore measures the inclination of these faces; but this angle is evidently twice the angle Ad_5C_1 (fig. 27*, Plate IV.*). What is true with regard to the angle of inclination over the edge C_1C_6 is true by similarity and symmetry of construction of all the other edges of the octahedron. And therefore the angle of inclination of any two adjacent faces of the octahedron is $109^\circ 28' 16''$.

$$26. \text{ Again (fig. 27*, Plate IV.*) } \tan AO_1D_5 = \frac{AD_5}{O_1D_5} = \sqrt{2}.$$

but $\tan Ad_5C_1 = \sqrt{2}$. Therefore $AO_1D_5 = Ad_5C_1$;

also $O_1AD_5 = 90^\circ - AO_1D_5 = 90^\circ - Ad_5C_1$; consequently

$AO_1d_5 = 90^\circ$, and the line AO_1 is perpendicular to C_1d_5 at the point o_1 .

By symmetry of construction the line O_1o_1 (fig. 14, Plate II.)

is perpendicular to the three lines C_1d_5 , C_2d_2 , and C_3d_1 , and consequently to the plane face $C_2C_2C_3$ of the octahedron.

Likewise by symmetry of construction each of the four axes O_1O_7 , &c., O_4O_6 , are respectively perpendicular to two opposite and parallel faces of the octahedron.

27. From triangle AO_1D_5 (fig. 27*) we have

$$AO_1^2 = O_1D_5^2 + AD_5^2 = 1 + 2 = 3.$$

Therefore $AO_1 = \sqrt{3}$.

In right-angled triangle C_1Ad_5 ; $C_1d_5^2 = C_1A^2 + Ad_5^2 = 1 + \frac{1}{2} = \frac{3}{2}$

Therefore $C_1d_5 = \sqrt{\frac{3}{2}}$.

But triangles AO_1d_5 and AD_5O_1 are similar.

$$\text{Therefore } \frac{AO_1}{Ad_5} = \frac{AD_5}{AO_1} \text{ and } AO_1 = \frac{Ad_5 \cdot AD_5}{AO_1} = \frac{\sqrt{2} \cdot \sqrt{\frac{3}{2}}}{\sqrt{3}} = \frac{\sqrt{3}}{3}$$

Consequently $AO_1 = \frac{1}{3}AO_1$.

Again by similar triangles Ad_5o_1 and C_1d_5A .

$$\frac{o_1d_5}{d_5A} = \frac{Ad_5}{d_5C_1} \quad o_1d_5 = \frac{(Ad_5)^2}{d_5C_1} = \frac{1}{3}\sqrt{\frac{3}{2}} = \frac{1}{3}C_1d_5. \quad \text{Also } Ad_5 = \frac{1}{2}AD_5.$$

28. Hence, referring to (fig. 14, Plate II.), we see that when the octahedron is inscribed in the cube, the three cubical axes, C_1C_6 , C_2C_4 , and C_3C_5 join together the opposite solid angles of the octahedron. The four octahedral axes O_1O_7 , O_2O_8 , &c., O_4O_6 , pass through the centres of two opposite faces of the octahedron and are perpendicular to them.

The points o_1 , o_2 , &c., being one-third of the distance of the centre of the cube from the solid angles O_1 , O_2 , &c., of the circumscribing cube.

Also that the six axes D_1D_{11} , &c., D_6D_8 , joining the opposite centres of the edges of the cube, pass each through two opposite edges of the inscribed octahedron. The distance of the centre of the cube from the centre of the edge of the octahedron being half the distance of the centre of the edge of the cube from that point.

29. Referring to fig. 27*, Plate IV.*, we have already shown, § 25, that the angle $Ad_5C_1 = \text{angle } AO_1D_5 = 54^\circ 44' 8''$, consequently, since $C_1AD_1O_1$ is by construction a parallelogram,

The angle $C_1AO_1 = 54^\circ 44' 8''$, and the angle $O_1AD_5 = 35^\circ 15' 52''$.

Hence the angle such as C_1AO_1 which any octahedral axis AO makes with any adjacent cubical axis AC is $54^\circ 44' 8''$; and the angle such as O_1AD_5 which the octahedral axis OA makes with any adjacent axis AD_5 is $35^\circ 15' 52''$. This latter axis is called a rhombic axis.

RHOMBIC AXES.

30. Describe a square $D_1C_1AC_2$ (fig. 28*, Plate IV.*) having its equal sides one-half the side or edge of the circumscribing cube. Join the diagonals C_1C_2 and D_1A meeting in d_1 . Produce D_1C_1 to O_1 and C_2A to D_5 , making C_1O_1 and AD_5 each $=AD_1$. Join C_1D_5 and O_1A meeting in o_1 . Draw O_1d_5 perpendicular to AD_5 . Then since $C_1O_1D_5A$ is a rectangular parallelogram, it follows AO_1 is bisected in o_1 , $o_1d_5 = \frac{1}{2}O_1D_5$ and $Ad_5 = \frac{1}{2}AD_5$.

Then referring to (fig. 12, Plate II.),—the square $C_1D_1C_2A$ represents on a plane surface (fig. 28*), and the parallelogram $C_1AD_5O_1$ the same figures shown in perspective in (fig. 12, Plate II.); the former being one-fourth of a section of the cube drawn through the points $D_1D_3D_{11}D_9$, and the latter one-fourth of the section drawn through $O_3O_1O_5O_7$.

$C_1d_1C_2$, C_1o_1 , o_1d_5 , &c., representing the lines similarly marked in the perspective figure of the rhombic dodecahedron inscribed in the cube.

31. Now fig. 30, Plate IV. Draw $C_2C_3 = C_1C_2$ (fig. 28*), on both sides C_2C_3 as base, describe two isosceles triangles having their equal sides, such as $C_2o_1 = C_1o_1$ (fig. 28*); join the diagonals C_2C_3 and o_1o_5 meeting in d_5 . $C_2o_5C_3o_1$ will represent on a plane surface a face of the rhombic dodecahedron, which can be inscribed in a cube whose edge is double C_2D_1 or O_1D_5 (fig. 27*).

32. (Fig. 28*, Plate IV.*) D_1d_1 is perpendicular to $C_1d_1C_2$, and also D_5d_5 is perpendicular to o_1d_5 . Hence, referring to (fig. 12, Plate II.), D_1d_1 is perpendicular to $C_1d_1C_2$, and D_5d_5 is perpendicular to o_1d_5 . Hence, by symmetry and similarity of construction, D_5d_5 is perpendicular to o_1o_5 , and C_2C_3 meeting in d_5 ; and therefore D_5d_5 is perpendicular to the face $o_1C_2o_5C_3$ of the rhombic dodecahedron, and passes through d_5 , its centre of gravity.

33. Hence by symmetry and similarity of construction comparing (fig. 12, Plate IV.) with (fig. 5, Plate I.), every axis D_1D_{11} , D_2D_{12} , D_3D_9 , &c., D_6D_8 , joining the opposite centres of the edges of the circumscribing cube, are each perpendicular to, and pass through the centres of gravity of opposite and parallel faces of the inscribed rhombic dodecahedron. Thus D_1D_{11} is perpendicular to $C_1o_1C_2o_4$, and $C_4o_6C_6o_7$, D_2D_{12} is perpendicular to $C_1o_1C_3o_2$ and $C_5o_8C_6o_7$, &c. From this property these axes are called the rhombic axes.

34. Again referring to (fig. 28*, Plate IV.*), we see that $AO_1 = \frac{1}{2}AO_1$ and $Ad_1 = \frac{1}{2}AD_1$. Hence by similarity and symmetry of construction (fig. 12, Plate II.) we see that the rhombic dodecahedron, inscribed in the cube, touches the centre of

each face of the cube, C_1, C_2 , &c., C_6 , by one of its four-faced solid angles; cuts each octahedral axis AO_1, AO_2 , &c., by o_1, o_2 , &c., one of its three-faced solid angles, at a distance AO_1 the $\frac{1}{2}$ of AO_1 . Also each semi-rhombic axis cuts the centre of the rhombic face, such as $C_2o_1C_3o_5$ at d_5 , Ad_5 being $\frac{1}{2}AD_5$.

To inscribe the three-faced Octahedron in the Cube.

35. (Fig. 29, Plate IV.) Describe the square $C_1D_1C_2A$, having each of its sides equal to O_1D_1 , fig. 27. Draw the diagonals C_1C_2 and D_1A meeting in d_1 .

Produce D_1C_1 and C_2A to O_1 and D_5 , make AD_5 and C_1O_1 each equal to AD_1 . Join O_1D_5 . In AD_5 take $Ad_5 = Ad_1$.

Produce AC_1 to M . For distance AM see § 37. Join d_5M , cutting AO_1 in o_1 . Then join C_1o_1 .

Then referring to (fig. 13, Plate II.), $C_1d_1C_2$ represents the edge of the three-faced octahedron, C_1o_1 and o_1d_5 the corresponding lines shown in perspective.

36. To draw the three-faced octahedron inscribed in the cube (fig. 27, Plate IV.).

Describe a square $O_1O_5O_8O_4$; draw O_4O_3 at such an angle and such a length that none of the edges or axes of the cube may obscure each other. Then draw O_1O_2, O_5O_6 , and O_8O_7 parallel and equal to O_4O_3 . Join O_3O_2, O_2O_6, O_6O_7 , and O_7O_3 . Also join O_1O_7, O_2O_8, O_3O_5 , and O_4O_6 meeting in A , the centre of the cube. These diagonals of the cube are the four octahedral axes of the cube.

Bisect O_1O_2 in D_1 , O_1O_2 in D_2 , &c., O_8O_7 in D_{12} ; join $D_1D_{11}, D_2D_{12}, D_3D_9, D_4D_{10}, D_5D_7$, and D_6D_8 , all intersecting in A . These are the six rhombic axes of the cube.

Lastly take C_1 the intersection of the diagonals of the face $O_1O_2O_3O_4$, C_2 that of the diagonals of the face $O_1O_5O_8O_4$, &c. Join C_1C_6, C_2C_4 , and C_3C_5 intersecting in A . These are the three cubical axes of the cube.

Then take a pair of proportional compasses and set them so that AO_1 (fig. 29, Plate IV.) be the distance between the shorter legs, and AO_1 between the longer legs of the compass.

Then in fig. 27, take the distance AO_1 with the longer legs and mark off AO_1 with the shorter; in the same way mark off the points o_2, o_3 , &c., o_8 , on the other octahedral axes.

Lastly (fig. 13, Plate II.) prick off from this construction of (fig. 27, Plate IV.) the points C_1, C_2 , &c., C_6 ; D_1, D_2 , &c., D_{12} ; O_1, O_2 , &c., O_8 ; and o_1, o_2 , &c., o_8 . Draw the same lines as in fig. 27.

Join C_1C_2, C_2C_3 , &c., $C_1o_1, C_2o_1, C_3o_1, C_1o_4, C_2o_4, C_5o_4$, &c. Then d_1, d_2 , &c., will be the points where the rhombic axes bisect the edges C_1C_2, C_1C_3 , &c. Join with dotted lines d_1o_1, d_2o_1 , &c.; then (fig. 13, Plate II.) will represent in perspective the three-faced octahedron inscribed in the cube.

In the solid itself the eight lines Oo are each equal O_1o_1 (fig. 29, Plate IV.), the twelve lines Dd are each equal D_1d_1 , or D_5d_5 (fig. 29).

37. The distance of the point M from A (fig. 29, Plate IV.) is arbitrary, so long as AM is greater than AO_1 .

For every point chosen for M , we have a value for AO_1 , which gives a distinct species of three-faced octahedron.

Speaking generally, taking AO_1 as a unit, AM may represent any whole number or fraction greater than unity.

The following values of AM have been observed in natural crystals:—

$AM = 2AO_1, \frac{3}{2}AO_1, 4AO_1, \frac{7}{4}AO_1, \frac{5}{4}AO_1$, and $\frac{65}{64}AO_1$.

38. Comparing (fig. 29, Plate IV.) with (fig. 27*, Plate IV.*), we see that M coincides with C_1 , and $AO_1 = \frac{AO_1}{3}$ for the octahedron;

and with (Plate IV.*, fig. 28*), $AO_1 = \frac{AO_1}{2}$ and o_1d_5 is parallel to AO_1 in the rhombic dodecahedron. In which case the point M is said to be at an infinite distance from A .

39. Hence referring to figs. 12, 13, and 14, Plate II., we see that the point o_1 of the three-faced octahedron cuts the octahedral axis at some point between $\frac{AO_1}{2}$ and $\frac{AO_1}{3}$; there being a distinct species of three-faced octahedron for every one of these points; the distance AO_1 , AO_2 , and AO_3 being the same for the same species.

40. Hence the rhombic dodecahedron, fig. 12, and the octahedron, fig. 14, are the two limiting forms of the three-faced octahedron.

41. If we construct (fig. 14) the edges of the cube in wire and all the lines of the octahedron, such as C_1d_5 , C_3d_1 , &c., in elastic threads; then if strings be fastened to o_1 tying together C_3d_1 , C_2d_2 , &c., and these strings pass over pulleys at the points o_1 , o_2 , &c., o_3 , if they be pulled uniformly so that o_1 , o_2 , &c., o_3 pass from $\frac{AO_1}{3}$ to $\frac{AO_1}{2}$ along the octahedral axes, the model will show in that finite space of time every one of the infinite number of species of three-faced octahedrons that can theoretically lie between fig. 14, the octahedron, and fig. 12, the rhombic dodecahedron inscribed in the cube.

Looking at the three figures, 12, 13, and 14, we see that the twelve lines, such as C_1d_5 , the edges of the octahedron, remain unaltered, the changing lines being represented by C_1o_1 and o_1d_5 . As the point o_1 travels from $\frac{AO_1}{3}$, fig. 14, to $\frac{AO_1}{2}$, fig. 12, the

apex o_1 rises from the triangular base $C_1C_2C_3$, in fig. 14, till two adjacent planes, fig. 12, over the edge $C_1d_1C_2$, such as $o_1C_1C_2$ and $o_4C_1C_2$, fig. 13, come into the same plane, fig. 12.

Fig. 14 having eight plane faces, passes through an infinite series of forms, such as fig. 13, bounded by 24 plane faces, and terminates fig. 12 in a form bounded by twelve plane faces.

42. If (fig. 32, Plate IV.) we draw $C_2C_3 = C_2C_1$ (fig. 29, Plate IV.), and describe on C_2C_3 the isosceles triangle $C_2o_1C_3$, having each of its equal sides C_2o_1 and $C_3o_1 = C_1o_1$ (fig. 29), then the triangle $C_2o_1C_3$ will represent, on a plane surface, one of the 24 equal faces of the three-faced octahedron which can be inscribed in a cube whose face is equal $O_1O_4O_8O_5$, fig. 27.

43. Twenty-four of these triangles drawn on a plane surface of cardboard can be cut out and folded together so as to make a model of the three-faced octahedron. Such drawings are called "nets." Nets ready drawn and fit for cutting and folding and making models for all the principal forms of crystals, by Mr. James B. Jordan, are published in Murby's Science and Art department Text Book, "Elementary Crystallography."

44. Referring to (Plate IV., fig. 29), we see that it is the distance of the point M from A which determines the point o_1 in AO_1 ; or referring to (fig. 13, Plate II.) the eight points $o_1, o_2, \&c., o_8$, which taken at equal distances from the centre of the circumscribing cube in the octahedral axes, determine the species of the three-faced octahedron. If (fig. 29, Plate IV.) we take AC_1 as unity and call $AM = m$, m then determines the species of the three-faced octahedron, m being any whole number or fraction greater than unity.

45. Now comparing (fig. 29, Plate IV.) with (fig. 13, Plate II.) we see that any particular face, such as $o_1C_2C_3$, cuts two cubical axes AC_2 and AC_3 in points C_2 and C_3 , and the third axis AO_1 produced in M , or at distances AC_2, AC_3 , and AM ; or 1, 1, and m . Since the line o_1d_5 cuts AC_1 in M , consequently the plane $o_1C_2C_3$ produced also cuts AC_1 in M . What is true for one face, by the similarity and symmetry of construction of the three-faced octahedron (fig. 13, Plate II.), is true for every

other of the 24 faces. If m be a fraction represented by $\frac{h}{k}$, then the following are the most received symbols for the three-faced octahedron.

$\frac{h}{k}O$ Naumann; $h\ h\ h$ Miller; and $a^{\frac{k}{h}}$ Brooke, Levy, and Des Cloizeau.

46. The following species have been observed in nature, having these respective values for m ; viz., 2, 3, $\frac{3}{2}$, 4, $\frac{7}{4}$, $\frac{5}{4}$, and $\frac{6.5}{4}$. The annexed table gives the respective symbols of the

principal crystallographers for these forms, together with the minerals in which faces of them have been found.

Naumann.	Miller.	Brooke, &c.	Minerals.
20	1 2 2	$a^{\frac{1}{2}}$	Amalgam. Fluor. Pharmaco- Argentite. Franklinite. siderite. Blende. Galena. Pyrite. Cuprite. Magnetite. Skutterudite. Diamond. Perowskite. Spinelle.
30	1 3 3	$a^{\frac{1}{3}}$	Cuprite. Fluor. Galena.
$\frac{3}{2}0$	2 3 3	$a^{\frac{2}{3}}$	Fahlerz. Garnet. Cuprite.
40	1 4 4	$a^{\frac{1}{4}}$	Galena. Kerate.
$\frac{2}{3}0$	4 7 7	$a^{\frac{4}{7}}$	Galena.
$\frac{5}{3}0$	4 5 5	$a^{\frac{4}{5}}$	Galena.
$\frac{6}{5}\frac{1}{4}0$	64 65 65	$a^{\frac{6}{5}\frac{1}{2}}$	Alum.

47. To find the ratio of the octahedral axis of the three-faced octahedron to that of the circumscribing cube, or of AO_1 to AO_1 .

Fig. 29, Plate IV. By construction $O_1D_5=1$ and $AD_5=\sqrt{2}$.

Therefore $\tan AO_1D_5=\sqrt{2}=54^\circ 44'$;

And therefore $O_1AD_5=35^\circ 16'$.

Also $\tan Md_5A=\frac{AM}{Ad_5}=\frac{m}{\frac{1}{2}\sqrt{2}}=m\sqrt{2}$.

But $AO_1d_5=180-(O_1Ad_5+Ad_5M)=180^\circ-35^\circ 16'-Ad_5M$.
 $=144^\circ 44'-Ad_5M$.

Hence $\sin AO_1d_5=\cos(90-AO_1d_5)=\cos(90-144^\circ 44'+Ad_5M)$.
 $=\cos(Ad_5M-54^\circ 44')$.

But in triangle AO_1d_5 , $\frac{AO_1}{Ad_5}=\frac{\sin Ad_5M}{\sin AO_1d_5}$

Therefore

$$\begin{aligned}
 AO_1 &= Ad_5 \frac{\sin Ad_5M}{\sin AO_1d_5} = Ad_5 \frac{\sin Ad_5M}{\cos(Ad_5M-54^\circ 44')} \\
 &= Ad_5 \frac{\sin Ad_5M}{\cos Ad_5M \cos 54^\circ 44' + \sin Ad_5M \sin 54^\circ 44'} \\
 &= Ad_5 \frac{\tan Ad_5M}{\cos 54^\circ 44' + \tan Ad_5M \sin 54^\circ 44'}
 \end{aligned}$$

But $Ad_5=\frac{\sqrt{2}}{2}=\frac{1}{\sqrt{2}}$ and $\tan Ad_5M=m\sqrt{2}$.

$$\sin 54^\circ 44' = \frac{AD_5}{AO_1} = \frac{\sqrt{2}}{\sqrt{3}} \text{ and } \cos 54^\circ 44' = \frac{O_1D_5}{AO_1} = \frac{1}{\sqrt{3}}$$

$$\begin{aligned} \text{Hence } AO_1 &= \frac{1}{\sqrt{2}} \cdot \frac{m\sqrt{2}}{\sqrt{\frac{1}{3}} + m\sqrt{2}\sqrt{\frac{2}{3}}} = \frac{m\sqrt{3}}{1+2m} \\ &= \frac{m}{1+2m} AO_1; \end{aligned}$$

$$\text{Or } \frac{AO_1}{AO_1} = \frac{m}{1+2m} = \frac{1}{1+1+\frac{1}{m}}$$

48. If we call the distances 1, 1, and m , at which each of the 24 faces of the three-faced octahedron if produced would cut three of the semi-cubical axes at right angles to each other, indices; then the ratio of $\frac{AO_1}{AO_1}$ = unity divided by the sum of the

reciprocals of the indices. Calling R this ratio, then when $m=2$ $R=\frac{2}{5}$; $m=3$ $R=\frac{3}{7}$; $m=\frac{3}{2}$ $R=\frac{2}{8}$; $m=4$ $R=\frac{4}{9}$; $m=\frac{7}{4}$ $R=\frac{7}{18}$; $m=\frac{5}{4}$ $R=\frac{5}{14}$; and $m=\frac{6.5}{4}$ $R=\frac{6.5}{19.4}$.

49. When $m=1$, the three-faced octahedron becomes the octahedron, and its three indices are 1, 1, and 1, and $R=\frac{1}{3}$.

Taking 1 1 m as the symbol for the three-faced octahedron, 1 1 1 must be taken as the symbol for the octahedron.

50. For the octahedron Naumann's symbol is O ; Miller's, 1 1 1; Brooke, Levy, and Des Cloizeau's a^1 .

51. When the third index becomes infinite, or, in other words, the face cuts two axes and is parallel to the third, then $m=\frac{1}{0}=\infty$, and $\frac{1}{m}=0$; and the three-faced octahedron is then the rhombic dodecahedron.

52. The three indices of the rhombic dodecahedron are, therefore, 1, 1, and ∞ ; and 1 1 ∞ becomes its symbol. Naumann's symbol is ∞O ; Miller's, 1 1 0; Brooke's, &c., b^1 .

To inscribe the four-faced Cube in the Cube.

53. (Fig. 37, Plate IV.) Describe the square $AC_1D_1C_2$ equal one-fourth of the square $O_1O_4O_8O_5$ (fig. 27, Plate IV.), this being a face of the cube in which the four-faced cube is to be inscribed. Join AD_1 (fig. 37, Plate IV.). Produce D_1C_1 to O_1 , and C_2A to D_5 . Make C_1O_1 and $AD_5=AD_1$. Join O_1D_5 .

Produce AC_1 to M , and make $AM=m$, m being any whole number or fraction greater than unity. The particular value of m will determine the particular species of the four-faced

cube, there being a distinct species for every value which can be assigned to m .

Join C_2M cutting AD_1 in d_1 . Join C_1d_1 , C_1d_2 .

In AD_5 take $Ad_5=Ad_1$. Draw d_5o_1 parallel to AM and cutting AO_1 in o_1 .

Join C_1o_1 .

Then (fig. 37, Plate IV.) represents the same lines and letters seen in perspective in (fig. 9, Plate II.), or the square $AC_1D_1C_2$ represents one-fourth of the section of the circumscribing cube through the centres of opposite edges of the cube, and the parallelogram $C_1O_1D_5A$ one-fourth of that through two opposite edges and two diagonals of opposite faces.

Taking, therefore, eight points, O_1o_1 , O_2o_2 , O_2o_3 , &c., O_8o_8 , in the octahedral axes of the circumscribing cube (fig. 9, Plate II.), each equal to O_1o_1 (fig. 37, Plate IV.) in the solid, or marking them in the perspective by proportional compasses as described in § 36. Join together C_1o_1 , C_1o_2 , C_2o_1 , C_2o_5 , &c.; and also o_1o_4 , o_1o_5 , &c., as in fig. 9, and we have the four-faced cube inscribed in the cube. Since in fig. 9, $o_1d_1=o_4d_4$, and D_1d_1 represents D_1d_1 , fig. 37, it is evident that every edge of the four-faced cube such as o_1o_4 is bisected by a rhombic axis D_1d_1 in the point d_1 .

54. If (fig. 34, Plate IV.) we draw $o_4d_1=o_1d_5$ (fig. 37), produce o_4d_1 to o_1 , and make $d_1o_1=d_1o_4$; on o_4o_1 as base describe an isosceles triangle $C_1o_4o_1$, having its equal sides C_1o_4 , C_1o_1 each $=C_1o_1$ (fig. 37).

Then $C_1o_4o_1$ will represent on a plane surface a face of the four-faced cube; and a net of 24 of these faces all equal to each other when folded up will form a solid four-faced cube, which can be accurately inscribed in a skeleton cube whose edges are all equal to O_1O_4 (fig. 9, Plate II.).

55. If we compare fig. 37, Plate IV., with fig. 9, Plate II., we see that o_1d_5 is parallel to AC_1 , and C_2d_1 cuts AC_1 produced in M , AM being taken equal to m . Hence, by similarity and symmetry of construction, we see that every face of the four-faced cube cuts one of the three cubical axes at a distance $=AC$, another at m times AC , and is parallel to the third. Hence, taking $AC=1$, then $1\ m\ \infty$ may be taken as the symbol for the four-faced cube.

Unity, m , and ∞ being the three indices of this form.

56. If m be represented as a fraction by $\frac{h}{k}$, then $\infty\ O\ m$ is

Naumann's symbol, $h\ k\ o$ Miller's, $\frac{h}{k}$ Brooke, Levy, and Des Cloizeau's.

57. $m=\frac{6}{5}$ occurs in crystals of pyrite; $m=\frac{5}{4}$ in perowskite; $m=\frac{4}{3}$ in diamond and perowskite; $m=\frac{3}{2}$ in argentite, blende,

diamond, pyrite, and perowskite; $m=2$ in argentite, copper, cobaltine, cuprite, fluor, gold, gersdorffite, garnet, magnetite, pyrite, percyllite, salt, and silver; $m=\frac{7}{3}$ in cubane; $m=\frac{5}{2}$ in copper and fluor; $m=3$ in amalgam, fahlerz, fluor, hauerite, and pyrite; $m=4$ in cobaltine and silver; $m=5$ in cuprite; $m=40$ in fluor.

58. When $m=1$, the symbol for the four-faced cube becomes $1\ 1\ \infty$, or the four-faced cube becomes the rhombic dodecahedron. When $m=\infty$, the symbol becomes $1\ \infty\ \infty$, which is that of the cube, each of whose faces cuts one of three cubical axes and is parallel to that of the other two.

59. Hence fig. 9, Plate II., shows that the four-faced cube is a form of an infinite number of species, the points such as o_1, o_2 , &c., in the octahedral axes lying between $\frac{1}{2}AO_1$ when it is the rhombic dodecahedron, and O_1 when it becomes the cube.

Constructing fig. 14, the skeleton cube, in wires, and the octahedron as shown with the lines passing through o and d in elastic strings, as before; then by pulling symmetrically all the points o_1, o_2 , &c., from $AO_1=\frac{1}{2}AO_1$ up to O_1 , all the forms of the four-faced cube, though infinite in number, will be represented to the eye in a finite space of time.

To obtain the Ratios of the Octahedral and Rhombic Axes of the four-faced Cube to those of the circumscribing Cube.

$$60. (\text{Fig. 37, Plate IV.}) \tan MC_2A = \frac{AM}{AC_2} = \frac{m}{1}$$

angle $D_1AC_2=45^\circ$ by construction.

$$\text{Hence in triangle } Ad_1C_2, d_1C_2A + C_2d_1A + 45^\circ = 180^\circ.$$

$$C_2d_1A = 135^\circ - d_1C_2A.$$

Therefore

$$\begin{aligned} \sin C_2d_1A &= \sin(135^\circ - d_1C_2A). \\ &= \cos(90^\circ - 135^\circ + d_1C_2A) = \cos(d_1C_2A - 45^\circ); \end{aligned}$$

$$\begin{aligned} \text{But in triangle } Ad_1C_2, \frac{Ad_1}{AC_2} &= \frac{\sin d_1C_2A}{\sin C_2d_1A} = \frac{\sin d_1C_2A}{\cos(d_1C_2A - 45^\circ)} \\ &= \frac{\sin d_1C_2A}{\sin d_1C_2A} \\ &= \frac{\cos d_1C_2A \cos 45^\circ + \sin d_1C_2A \sin 45^\circ}{\tan d_1C_2A} = \frac{m\sqrt{2}}{\sqrt{\frac{1}{2}}(1 + \tan d_1C_2A)} = \frac{m\sqrt{2}}{1+m} \end{aligned}$$

$$\text{But } AC_2=1 \text{ and } AD_1=\sqrt{2}.$$

$$\text{Therefore } Ad_1 = \frac{m}{1+m} AD_1.$$

But $Ad_5=Ad_1$ and $AD_5=AD_1$ and o_1d_5 is parallel to O_1D_5 .

$$\text{Therefore } \frac{AO_1}{AO_1} = \frac{Ad_5}{AD_5} = \frac{m}{1+m} \text{ or } AO_1 = \frac{m}{1+m} AO_1.$$

Hence we see that the ratios of the octahedral and rhombic axes of the inscribed four-faced cube to those of the circumscribing cube are each equal to $\frac{m}{1+m}$. Calling this ratio R , and

putting it under the form $R = \frac{1}{1 + \frac{1}{m}}$; we see that for the cube

$m = \infty$, $R = 1$; and for the rhombic dodecahedron $m = 1$, and therefore $R = \frac{1}{2}$.

Hence for the four-faced cube R varies from 1 to $\frac{1}{2}$.

When $m = \frac{6}{5}$, $R = \frac{6}{11}$; $m = \frac{5}{4}$, $R = \frac{5}{9}$; $m = \frac{4}{3}$, $R = \frac{4}{7}$;
 $m = \frac{3}{2}$, $R = \frac{3}{5}$; $m = 2$, $R = \frac{2}{3}$; $m = \frac{7}{3}$, $R = \frac{7}{10}$;
 $m = \frac{5}{2}$, $R = \frac{5}{7}$; $m = 3$, $R = \frac{3}{4}$; $m = 4$, $R = \frac{4}{5}$;
 $m = 5$, $R = \frac{5}{6}$; $m = 40$, $R = \frac{40}{41}$.

61. *To inscribe the twenty-four faced trapezohedron in the cube.*

(Fig. 31, Plate IV.) Describe the square $AC_1D_1C_2$ = one-fourth the face of the cube $O_1O_5O_8O_4$ (fig. 27). Join AD_1 . Produce D_1C_1 to O_1 , and C_2A to D_5 . Make C_1O_1 and AD_5 each = AD_1 . Join O_1D_5 . Produce AC_1 to M_1 , and take $AM = m$, AC_1 being 1, and m any whole number or fraction greater than unity. m determines the particular species of the twenty-four-faced trapezohedron.

Join C_2M meeting AD_1 in d_1 . In AD_5 take $Ad_5 = Ad_1$.

Join d_5M cutting AO_1 in o_1 . Join C_1o_1 and C_1d_1 .

Then in (fig. 11, Plate II.), describe fig. 27, Plate IV., and take the eight points, o_1 , o_2 , &c., o_8 , in the octahedral axes so that $\frac{Ao_1}{AO_1} = \frac{Ao_2}{AO_2}$ (fig. 11), $= \frac{Ad_1}{AO_1}$ (fig. 31, Plate IV.). And the twelve points d_1 , d_2 , &c., d_{12} , in fig. 11, Plate II., so that $\frac{Ad_1}{AD_1} = \frac{Ad_2}{AD_2} = \frac{Ad_1}{AD_1}$ (fig. 31, Plate IV.), as described in § 36.

Then joining the points C , d , and o , as shown in (fig. 11, Plate II.) the twenty-four-faced trapezohedron will be inscribed in the cube.

62. If (fig. 39, Plate IV.) we describe a triangle having one of its sides $C_1o_1 = C_1o_1$ (fig. 31), another side $C_1d_1 = C_1d_1$ (fig. 31), and its third side $o_1d_1 = o_1d_5$ (fig. 31);

Then, on the other side of the base C_1o_1 (fig. 39), describe the triangle $C_1d_2o_1$ similar and equal to the triangle $C_1d_1o_1$.

$C_1d_1o_1d_2$ will represent on a plane surface a face of the twenty-four-faced trapezohedron, and 24 of these faces, formed into a net and folded together will make a solid twenty-four-faced trapezohedron, which can be inscribed with

a skeleton cube whose face = $O_1O_5O_8O_4$, fig. 27, in the position shown in (fig. 11, Plate II.).

63. Since (fig. 31) O_2d_1 cuts AM in M , and d_5o_1 cuts AO_1 also in M , and comparing this with fig. 11, Plate II., we see that every face of the twenty-four-faced trapezohedron cuts one cubical axis at a distance equal AO_1 , and two other cubical axes at m times this distance.

Taking AO_1 as unity, we see that the three indices of the twenty-four-faced trapezohedron are 1, m , and m . Its symbol, therefore, is 1, m , m .

Representing m as a fraction by $\frac{h}{k}$, Naumann's symbol is $m O m$; Miller's h, k, k ; Brooke, Levy, and Des Cloizeau's $h k$.

64. $m = \frac{4}{3}$ occurs in crystals of galena and garnet; $m = \frac{3}{2}$ in argentite, gold, and tennantite; $m = 2$ in amalgam, argentite, analcime, boracite, cuprite, dufrenoyite, eulytine, fahlerz, franklinite, fluor, gold, galena, garnet, leucite, pyrite, pyrochlore, sal-ammoniac, sodalite, smaltine, and tennantite; $m = \frac{9}{4}$ in perowskite; $m = \frac{8}{3}$ in fluor; $m = 3$ in blende, copper, fahlerz, fluor, gold, galena, magnetite, pyrite, perowskite, pyrochlore, and spinelle; $m = 4$ in sal-ammoniac and kerate; $m = 5$ in galena; $m = 6$ in magnetite; $m = 10$ in magnetite; $m = 12$ in blende; $m = 16$ in galena and magnetite; $m = 40$ in pharmacosiderite.

65. *To find the ratios of the rhombohedral and octahedral axes of the twenty-four-faced trapezohedron to those of the circumscribing cube.*

The right-hand side of the (fig. 31, Plate IV.) being the same by construction as that of (fig. 37, Plate IV.) for the four-faced cube.

$$Ad_1 = \frac{1}{1 + \frac{1}{m}} AD_1, \text{ or } \frac{Ad_1}{AD_1} = \frac{m}{m+1} \text{ as in § 60.}$$

$$\text{But fig. 31, } Ad_5 = Ad_1 = \frac{m}{m+1} \sqrt{2}.$$

$$\tan Ad_5M = \frac{AM}{Ad_5} = m \frac{m+1}{m\sqrt{2}} = \frac{m+1}{\sqrt{2}}$$

$$\text{but } \sin O_1Ad_5 = \frac{O_1D_5}{AO_1} = \frac{1}{\sqrt{3}} \text{ and } \cos O_1Ad_5 = \frac{AD_5}{AO_1} = \frac{\sqrt{2}}{\sqrt{3}}$$

$$\text{also } \sin Ao_1d_5 = \sin \{180^\circ - (Ad_5M + O_1Ad_5)\}.$$

$$= \sin (Ad_5M + O_1Ad_5).$$

$$= \sin Ad_5M \cos O_1Ad_5 + \cos Ad_5M \sin O_1Ad_5.$$

But in triangle AO_1d_5 $\frac{AO_1}{Ad_5} = \frac{\sin Ad_5M}{\sin AO_1d_5}$

Therefore $AO_1 = \frac{\sin Ad_5M}{\sin AO_1d_5} Ad_5$.

$$\begin{aligned}
 &= \frac{\sin Ad_5M \cdot m\sqrt{2}}{(m+1)(\sin Ad_5M \cos O_1Ad_5 + \cos Ad_5M \sin O_1Ad_5)} \\
 &= \frac{m\sqrt{2}}{(m+1)(\cos O_1Ad_5 + \cot Ad_5M \sin O_1Ad_5)} \\
 &= \frac{m\sqrt{2}}{(m+1)\left\{\frac{\sqrt{2}}{\sqrt{3}} + \frac{\sqrt{2}}{m+1} \cdot \frac{1}{\sqrt{3}}\right\}} = \frac{m\sqrt{3}}{m+1+1} \\
 &= \frac{1}{1 + \frac{1}{m} + \frac{1}{m}} AO_1.
 \end{aligned}$$

Hence the ratio of the rhombic axes of the twenty-four-faced trapezohedron to those of circumscribing cube, or $\frac{Ad_1}{AD_1} = \frac{1}{1 + \frac{1}{m}}$; and the ratio of the octahedral axes of the

twenty-four faced trapezohedron, or $\frac{AO_1}{AO_1} = \frac{1}{1 + \frac{1}{m} + \frac{1}{m}}$

66. Representing $\frac{Ad_1}{AD_1}$ as R_1 , and $\frac{AO_1}{AO_1}$ as R_2 .

R_1 =unity divided by the sum of the reciprocals of the first two indices taken in order of magnitude, and

R_2 =unity divided by the sum of the reciprocals of the three indices.

When $m = \frac{4}{3}$	$R_1 = \frac{4}{7}$	and $R_2 = \frac{2}{5}$
$m = \frac{3}{2}$	$R_1 = \frac{3}{5}$	$R_2 = \frac{3}{7}$
$m = 2$	$R_1 = \frac{2}{3}$	$R_2 = \frac{1}{2}$
$m = \frac{9}{4}$	$R_1 = \frac{9}{13}$	$R_2 = \frac{9}{17}$
$m = \frac{8}{3}$	$R_1 = \frac{8}{11}$	$R_2 = \frac{4}{7}$
$m = 3$	$R_1 = \frac{3}{4}$	$R_2 = \frac{3}{5}$
$m = 4$	$R_1 = \frac{4}{5}$	$R_2 = \frac{2}{3}$
$m = 5$	$R_1 = \frac{5}{6}$	$R_2 = \frac{5}{7}$
$m = 10$	$R_1 = \frac{10}{11}$	$R_2 = \frac{5}{6}$
$m = 12$	$R_1 = \frac{12}{13}$	$R_2 = \frac{6}{7}$
$m = 16$	$R_1 = \frac{16}{17}$	$R_2 = \frac{8}{9}$
$m = 40$	$R_1 = \frac{40}{41}$	$R_2 = \frac{20}{21}$

67. When $m=1$, $R_1=\frac{1}{2}$, and $R_2=\frac{1}{3}$, and the twenty-four-faced trapezohedron becomes the octahedron.

When $m=\infty$ and $\frac{1}{m}=0$, $R_1=1$ and $R_2=1$, and the twenty-four-faced trapezohedron becomes the cube.

Hence, referring to (fig. 11, Plate II.) we see that the twenty-four-faced trapezohedron is a variable form of an infinite number of species, varying from the octahedron as one limit to the cube as the other.

If we represent this passage as in the instances of the three-faced octahedron, § 41, and four-faced cube, § 59, we must raise the eight points o_1, o_2 , &c., o_8 , from o_1 equal $\frac{1}{3} AO_1$ in the octahedron (fig. 14) to O_1 , fig. 8; at the same time raising the points d_1, d_2 , &c., d_{12} along the lines AD_1, AD_2 , &c., from d_1, d_2 , &c. (fig. 14), equal one-half AD_1 , to the point D_1, D_2 , &c. (fig. 8); taking care that the point d shall have such a relation to o that two adjacent triangles on each side of Co are in the same plane.

68. *To inscribe the six-faced octahedron in the cube.*

(Fig. 35, Plate IV.) Describe the square $AC_1D_1C_2$ equal one-fourth of the square $O_1O_4O_8O_5$ (fig. 27). Join AD_1 . Produce D_1C_1 to O_1 , and C_2A to D_5 , making C_1O_1 and AD_5 equal AD_1 . Join O_1D_5 . Produce AC_1 to M and N . Taking $AC_1=1$, make $AM=M$ and $AN=n$; m being any whole number or fraction greater than unity, and n any whole number or fraction greater than m .

Join C_2M , cutting AD_1 in d_1 . Take $AD_5=Ad_1$. Join d_5N , cutting AO_1 in o_1 . Join C_1o_1 .

Then, in fig. 27, take 12 points, d_1, d_2 , &c., d_{12} , in AD_1, AD_2 , &c., AD_{12} , so that $\frac{Ad_1}{AD_1}, \frac{Ad_2}{AD_2}$, &c., $\frac{Ad_{12}}{AD_{12}}$ are each equal to $\frac{Ad_1}{AD_1}$, fig. 35, which can be easily done with proportional compasses.

Also, in fig. 27, take eight points, o_1, o_2 , &c., o_8 , in AO_1, AO_2 , &c., AO_8 , so that $\frac{Ao_1}{AO_1}, \frac{Ao_2}{AO_2}$, &c., $\frac{Ao_8}{AO_8} = \frac{Ao_1}{AO_8}$, fig. 35.

Join the points C, d , and o as in (fig. 10, Plate II.), and the six-faced octahedron inscribed in the cube will be shown in perspective. In a model showing the solid six-faced octahedron inscribed in a skeleton cube, each of the lines O_1o_1, O_2o_2 , &c., O_8o_8 , will be equal O_1o_1 fig. 35, and each of the lines D_1d_1, D_2d_2 , &c., $D_{12}d_{12}$, will be equal D_1d_1 , fig. 35.

69. Fig. 36, Plate IV. Draw a triangle, $C_1o_1d_2$, such that C_1o_1 , fig. 36, = C_1o , fig. 35; C_1d_2 , fig. 36, = C_1d_1 , fig. 35; and o_1d_2 , fig. 36, = o_1d_5 , fig. 35.

Then $C_1O_1d_2$ (fig. 36) is a face on a plane surface of the six-faced octahedron which can be inscribed in a cube, each of whose faces are equal $O_1O_4O_8O_5$, fig. 27.

Forty-eight triangles, similar and equal to $C_1O_1d_2$, arranged as a *net* and cut out of cardboard, will fold up into a solid model of the six-faced octahedron.

70. Each face of the six-faced octahedron, if produced, cuts one axis of the cube at the distance $=1$, another at the distance $=m$, and the third at a distance n from the centre of the cube.

The three quantities, 1, m , and n are termed the three indices of the six-faced octahedron.

Its symbol, therefore, is 1, m, n ; Naumann's symbol is nOm .

If the three fractions $1, \frac{1}{m}, \frac{1}{n}$ be brought to a common denominator, and the three numerators divided, if they possess any common factor, by that factor, be represented by h, k, l , these being whole numbers, then h, k, l is Miller's symbol, and $b^{\frac{1}{h}}b^{\frac{1}{k}}b^{\frac{1}{l}}$ is that of Brooke, Lévy, and Des Cloizeau.

71. The form $1, \frac{64}{3}, 64$ occurs in garnet; $1, \frac{5}{4}, \frac{5}{3}$ in pyrite and gold; $1, \frac{4}{3}, 2$ in linneite; $1, \frac{4}{3}, 4$ in garnet; $1, \frac{15}{11}, \frac{15}{7}$ in linneite; $1, \frac{3}{2}, 3$ in amalgam, cobaltine, cuprite, diamond, fahlerz, garnet, hauerite, magnetite, and pyrite; $1, \frac{8}{5}, 8$ in pyrite; $1, \frac{5}{3}, 5$ in boracite and pyrite; $1, \frac{5}{3}, 10$ in pyrite; $1, 2, 4$ in fluor, gold, and pyrite; $1, 2, 10$ in pyrite; $1, \frac{11}{5}, \frac{11}{3}$ in fluor; $1, \frac{16}{7}, 4$ in fluor; $1, \frac{7}{3}, 7$ in fluor; $1, 3, \frac{21}{5}$ in magnetite; $1, 4, 8$ in galena.

72. To find the ratios of the rhombohedral and octahedral axes of the six-faced octahedron to those of the circumscribing cube.

In fig. 35, Plate IV., the sides of the square $AC_1D_1C_2$ are by construction equal to unity. Hence $AD_1 = \sqrt{2}$, and angle $D_1AC_2 = 45^\circ$. Also $AM = m$ by construction. Let angle $AC_2d_1 = a$. Then $Ad_1C_2 = 180^\circ - (a + 45)$.

$$\text{Then } \cos a = \frac{AC_2}{m} = \frac{1}{m},$$

and in triangle Ad_1C_2 ,

$$\frac{Ad_1}{AC_2} = \frac{\sin a}{\sin \{180 - (a + 45)\}} = \frac{\sin a}{\sin (a + 45)}$$

$$\begin{aligned} Ad_1 &= \frac{\sin a}{\sin a \cos 45 + \cos a \sin 45} = \frac{1}{\sqrt{\frac{1}{2}} + \sqrt{\frac{1}{2}} \cos a} \\ &= \frac{\sqrt{2}}{1 + \frac{1}{m}} = \frac{1}{1 + \frac{1}{m}} AD_1. \end{aligned}$$

Therefore $\frac{Ad_1}{AD_1} = \frac{1}{1 + \frac{1}{m}}$.

Hence the ratio of each rhombic axis of the six-faced octahedron to that of the circumscribing cube is $\frac{1}{1 + \frac{1}{m}}$, or of unity divided by the sum of the reciprocals of the two smaller indices of the six-faced octahedron.

73. Again in (fig. 35, Plate IV.), in the parallelogram $C_1O_1D_5A_1$, $C_1A = O_1D_5 = 1$, and $C_1O_1 = AD_5 = \sqrt{2}$; also $Ad_5 = Ad_1 = \frac{\sqrt{2}}{1 + \frac{1}{m}}$.

Let $\gamma = O_1AD_5$ and $\beta = Ad_5N$. Then $AO_1d_5 = 180^\circ - (\beta + \gamma)$.

But $AO_1^2 = O_1D_5^2 + AD_5^2 = 1 + 2 = 3$.

and $AO_1 = \sqrt{3}$.

Also $\sin \gamma = \frac{O_1D_5}{AO_1} = \frac{1}{\sqrt{3}}$ and $\cos \gamma = \frac{AD_5}{AO_1} = \frac{\sqrt{2}}{\sqrt{3}}$.

In triangle NAd_5 $\cot \beta = \frac{Ad_5}{AN} = \frac{Ad_5}{n}$.

Also in triangle AO_1d_5 .

$$\begin{aligned} \frac{AO_1}{Ad_5} &= \frac{\sin \beta}{\sin \{180 - (\beta + \gamma)\}} = \frac{\sin \beta}{\sin (\beta + \gamma)} \\ &= \frac{\sin \beta}{\sin \beta \cos \gamma + \cos \beta \sin \gamma} = \frac{1}{\cos \gamma + \cot \beta \sin \gamma} \end{aligned}$$

$$\begin{aligned} \text{Hence } AO_1 &= \frac{Ad_5}{\frac{\sqrt{2}}{\sqrt{3}} + \frac{Ad_5}{n} \cdot \frac{1}{\sqrt{3}}} = \frac{\frac{\sqrt{3}}{\sqrt{2}} + \frac{1}{n}}{\frac{\sqrt{3}}{\sqrt{2}} + \frac{1}{n}} \\ &= \frac{\sqrt{3}}{\left(\frac{1 + \frac{1}{m}}{\sqrt{2}}\right) \sqrt{2} + \frac{1}{n}} = \frac{AO_1}{1 + \frac{1}{m} + \frac{1}{n}} \end{aligned}$$

$$\text{And } \frac{AO_1}{AO_1} = \frac{1}{1 + \frac{1}{m} + \frac{1}{n}}$$

Hence ratio of the octahedral axis of six-faced octahedron is to that of the circumscribing cube as $\frac{1}{1 + \frac{1}{m} + \frac{1}{n}}$, or unity

divided by the sum of the reciprocals of its three parameters.

74. Let $R_1 = \frac{Ao}{AO}$, and $R_2 = \frac{Ad}{AD}$

For the form	1	$\frac{6}{6}\frac{4}{3}$	64	$R_1 = \frac{6}{12}\frac{4}{7}$	$R_2 = \frac{1}{2}$
	1	$\frac{5}{4}\frac{4}{3}$	$\frac{5}{3}$	$R_1 = \frac{5}{9}$	$R_2 = \frac{5}{12}$
	1	$\frac{4}{3}\frac{4}{3}$	2	$R_1 = \frac{4}{7}$	$R_2 = \frac{4}{9}$
	1	$\frac{4}{3}\frac{4}{3}$	4	$R_1 = \frac{4}{7}$	$R_2 = \frac{1}{2}$
	1	$\frac{1}{1}\frac{5}{1}$	$\frac{1}{7}\frac{5}{7}$	$R_1 = \frac{1}{2}\frac{5}{6}$	$R_2 = \frac{1}{3}\frac{5}{3}$
	1	$\frac{3}{2}\frac{4}{3}$	3	$R_1 = \frac{3}{5}$	$R_2 = \frac{1}{2}$
	1	$\frac{8}{5}\frac{4}{3}$	8	$R_1 = \frac{8}{13}$	$R_2 = \frac{4}{7}$
	1	$\frac{5}{3}\frac{4}{3}$	5	$R_1 = \frac{5}{8}$	$R_2 = \frac{5}{9}$
	1	$\frac{5}{3}\frac{4}{3}$	10	$R_1 = \frac{5}{8}$	$R_2 = \frac{1}{17}$
	1	2	4	$R_1 = \frac{2}{3}$	$R_2 = \frac{4}{7}$
	1	2	10	$R_1 = \frac{2}{3}$	$R_2 = \frac{5}{8}$
	1	$\frac{11}{5}\frac{4}{3}$	$\frac{11}{3}$	$R_1 = \frac{11}{16}$	$R_2 = \frac{1}{19}$
	1	$\frac{16}{7}\frac{4}{3}$	4	$R_1 = \frac{16}{23}$	$R_2 = \frac{16}{27}$
	1	$\frac{7}{3}\frac{4}{3}$	7	$R_1 = \frac{7}{10}$	$R_2 = \frac{7}{11}$
	1	3	$\frac{2}{5}\frac{1}{5}$	$R_1 = \frac{3}{4}$	$R_2 = \frac{7}{11}$
	1	4	8	$R_1 = \frac{4}{5}$	$R_2 = \frac{8}{11}$

75. Referring now to (Plate II., fig. 10), we may observe that the six-faced octahedron is the form from which all the others represented on that plate are derived.

76. When the indices m and n are equal, and both greater than unity, the six-faced octahedron (fig. 10) becomes the twenty-four-faced trapezohedron, fig. 11, in which case two adjacent faces over the edge Co become in the same plane, and the 48 faces of the six-faced octahedron are reduced to the 24 faces of the twenty-four-faced trapezohedron.

77. When the index n becomes infinite, and m is some number or fraction greater than unity, the six-faced octahedron becomes the four-faced cube (fig. 9), and two adjacent planes over the edge Cd become in the same plane, and so the 48 faces of the six-faced octahedron are reduced to the 24 faces of the four-faced cube.

78. When the index m becomes unity, and n is some number or fraction greater than unity, the six-faced octahedron becomes the three-faced octahedron (fig. 13), and two adjacent faces over the edge od become in the same plane, and so the 48 faces of the six-faced octahedron are reduced to the 24 faces of the three-faced cube.

79. When the two indices m and n are both equal to unity, the six-faced octahedron becomes the octahedron (fig. 14), and the six faces round each octahedral axis become in the same plane, and the 48 faces of the six-faced octahedron are reduced to the eight faces of the octahedron.

80. When the index $m =$ unity, and n becomes infinite, the six-faced octahedron becomes the rhombic dodecahedron (fig. 12), and the four faces surrounding the rhombic axes are

in the same plane, and the 48 faces of the six-faced octahedron are reduced to the twelve faces of the rhombic dodecahedron.

81. When both the indices m and n become infinite, the six-faced octahedron becomes the cube fig. 8, and the eight faces surrounding the cubical axes are in the same plane, and the 48 faces of the six-faced octahedron are reduced to six faces of the cube.

82. By giving the necessary values to m and n , the formulæ belonging to any of the forms in Plate II. may be derived from those calculated for the six-faced octahedron. If fig. 10 be constructed, the outlines of the circumscribing cube in wire, and the 48 triangles Cdo in elastic strings fastened to the skeleton cube at C , and strings tying together the lines CdO and odo at d , and the four strings Cd meeting in o , and these be made to pass over pulleys at D and O ; then by a proper adjustment of the lengths of Oo and Dd , taking care that the eight lines Oo and the twelve lines Dd are the same in length for each particular form,—the 48 triangles of the elastic six-faced octahedron may be made to assume the shape of any holohedral form of the cubical system.

83. Whenever faces parallel to different forms of crystals occur in the same crystal, such as is shown in a crystal of native copper (fig. 29*, Plate IV.*), these faces are always parallel to those of their respective forms when inscribed in a cube, every other form having the same invariable position with respect to the cube, as shown in (Plate II.) Faces parallel to those of the cube are marked C_1, C_2, C_3 ; octahedron o_1, o_4, o_8, o_5 ; rhombic dodecahedron d_1, d_2, d_3 , &c., and H_1, H_2 , &c., those of a four-faced cube are all shown on the same crystal.

84. It will also be seen by reference to (fig. 29), that the intersections of the faces of the crystal or the edges between $C_1, H_6, d_1, H_5, C_2, H_8, d_9$, and H_9 are lines parallel to one another, as also are those of $C_3, H_1, d_5, H_5, C_2, H_7, d_8, H_{12}$. Faces whose intersections are thus parallel are said to belong to the same zone, for a reason to be shown presently.

85. (Fig. 30*, Plate IV.*) Let the three planes $CDGH$, $DEKH$, and $EFLK$ be perpendicular to the plane $G HKL$, intersecting it in the lines GH , HK , and KL . From A , a point in the plane $G HKL$, draw AM perpendicular to GH , AN to HK , and AO to KL . Through A draw AB perpendicular to the plane $G HKL$. Then it may be easily shown by the Eleventh Book of Euclid, that CG , DH , EK , and FL are parallel to AB ; also that AM is perpendicular to the plane $CDHG$, AN to $DEKH$, and AO to $EKLF$. Also DH perpendicular to GH and HK , and EK perpendicular to KH and KL .

AM , AN , and AO are called normals from the point O to the plane to which they are respectively perpendicular.

Now the inclination of the plane $CDHG$ to the plane $DHEK$ over their intersecting edge DH is measured by the angle MHN , MH and HN being drawn through the point H , perpendicular in each of the planes to their common intersection DH . Similarly the angle NKO measures the inclination of the plane $DEKH$ to the plane $EKLH'$ over the edge of their intersection EK .

In every quadrilateral lineal figure drawn in the same plane the four angles of the figure are always equal to four right angles, and in the plane $GHLK$ the angles AMH , ANH , ANK , and AOK are all right angles. Hence the angle $MHN = 180^\circ - MAN$, and the angle $NKO = 180^\circ - NAO$.

In other words, the *normals* drawn through a point perpendicular to two intersecting planes, make with each other an angle which is the supplement to that which measures the inclination of these planes to each other over their intersecting edge.

86. The power of representing the combination of faces of crystals with each other such as (fig. 29*, Plate IV.*) is necessarily limited to those of comparatively few faces. But, taking advantage of the relationship of the inclination of faces of crystals measured over their edges of intersection to that of their normals drawn from a certain point within the crystal, Professor Neumann, of Königsberg, devised a system by which the relationship of all the forms of any number of crystals might be graphically represented at one view.

For instance, to represent the relationship of all the forms of the cubical system to each other, we suppose the cube (fig. 27, Plate IV.) to be inscribed in a sphere whose centre corresponds with A , the centre of the cube. From this centre A , normals are drawn perpendicular to every face of the cube, and to those of every form which can be inscribed in it.

The points where these normals cut the surface of the circumscribing sphere are called the poles of their respective faces, and the arc of the great circle between any two poles is the supplement of that arc which measures the inclination of their respective faces over the straight edge of their intersection.

87. Referring to (fig. 27, Plate IV.), we see that AC_1 and AC_2 , the normals of opposite faces of the cube, are in the same straight line, as also are AC_2 and AC_4 , AC_3 and AC_5 ; also that the three axes C_1C_6 , C_2C_4 , and C_3C_5 are perpendicular to each other. The six equal lines AC_1 , AC_2 , &c., AC_6 are equal radii of a sphere, which can be inscribed in the cube, having A for its centre and touching the six faces of the cube in their poles, C_1 , C_2 , &c., C_6 .

Upon this sphere we may project the poles of all the faces of the different forms (fig. 9 to fig. 14, Plate II.), which can be inscribed in the cube.

Let (fig. 31* and fig. 32*, Plate IV.*) represent the projections of two hemispheres of this sphere upon the plane of the paper.

Let C_1C_6 and C_5C_3 (fig. 31*) be two diameters intersecting at right angles in C_2 . Also C_1C_6 and C_5C_4 (fig. 32*) be two diameters intersecting at right angles in C_4 .

Then C_1, C_2, C_3 , &c., C_6 , represent the poles of the six faces of the cube on the sphere of projection. Also the eight equilateral spherical triangles $C_1C_2C_3, C_1C_5C_2, C_5C_2C_6$, &c., divide the sphere of projection into eight equal octants.

88. Bisect each of the twelve arcs $C_1C_2, C_1C_3, C_1C_4, C_1C_5$, &c., by the points D_1, D_2, D_3 , and D_{12} ; these twelve points will be the twelve poles of the rhombic dodecahedron on the sphere of projection (figs. 31* and 32*, Plate IV.*), or the twelve points where the rhombic axes AD_1, AD_2, AD_3, AD_4 , &c., of fig. 27 cut the surface of the sphere of projection inscribed in the cube.

89. Join C_1D_5, C_2D_2, C_3D_1 by arcs of great circles meeting in O_1 ; this will divide the octant of the sphere $C_1C_2C_3$ into six equal and similar spherical triangles. Let this be done to each of the other octants. Then (fig. 31* and fig. 32*, Plate IV.*) the eight points O_1, O_2 , &c., O_8 , will represent the eight poles of the octahedron on the sphere of projection.

The sphere of projection is thus divided into 48 equal and similar but right and left-handed spherical triangles, indicated by the triangles COD , with different indices to the letters.

90. Any great circle of the sphere of projection is called a zone circle, and the poles of all faces which are in that great circle are said to lie in the same zone, and their intersections will be parallel to each other (see § 84 and 85).

91. We see in (fig. 9, Plate II.) that the normal to any face such as $C_1o_1o_2$, must, by the symmetry of construction of the four-faced cube, pass through some point in the line C_1d_2 . Hence in the sphere of projection (figs. 31* and 32*, Plate IV.*), the 24 poles of any four-faced cube will lie in each of the 24 arcs CD .

92. The normals to any face of the twenty-four-faced trapezohedron, such as $C_1d_1o_1d_2$ (fig. 11, Plate II.), must, by symmetry of construction, pass through the line C_1o_1 . Hence in the sphere of projection (figs. 31* and 32*, Plate IV.*), the

24 poles of any twenty-four-faced trapezohedron will lie in each of the 24 arcs CO .

93. The normals to any face of the three-faced octahedron (fig. 13, Plate II.), such as $C_1O_1C_2$, must, by symmetry of construction, pass through the line d_1O_1 . Hence in the sphere of projection, (figs. 31 and 32, Plate IV.), the 24 poles of the three-faced octahedron will lie in each of the arcs DO .

94. Hence in the same zone $C_1D_1C_2D_9C_6D_{11}C_4D_3$ there will be four poles of the cube, C_1, C_2, C_6, C_4 ; four poles of the rhombic dodecahedron, D_1, D_9, D_{11}, D_3 ; and eight poles of the four-faced cube.

The same will be true of the two zones $C_2D_5C_3$ and $C_3D_2C_1$.

Again in the zone $C_3O_1D_1O_4C_5O_6D_{11}O_7C_5$, there will be two poles of the cube, C_3 and C_5 , two poles of the rhombic dodecahedron, D_1 and D_{11} , four of the octahedron, O_1, O_4, O_6 , and O_7 , four of the three-faced octahedron, and also four of the twenty-four-faced trapezohedron, will lie.

The same will also be true for the five other zones, $C_3O_5D_9, C_1O_1D_5, C_1O_4D_3, C_2O_1D_2$, and $C_2O_4D_4$.

95. The 48 poles of any six-faced octahedron will, from the symmetry of its construction, occupy similar positions within the 48 spherical triangles CDO (figs. 31* and 32*, Plate IV.*).

96. In each of the 48 spherical triangles CDO (figs. 31 and 32, Plate IV.*) is marked a notation for each of the 48 poles of the six-faced octahedron in terms of its three indices. The order in which the three indices l, m , and n are written, mark the distances at which the face of the six-faced octahedron corresponding to the pole marked on the sphere of projection, cuts each of three cubical axes taken in the order AC_3, AC_2 , and AC_1 (fig. 27, Plate IV.). When the index has a negative sign placed over it, it signifies that it cuts the axis AC_3 produced in the direction AC_5, AC_2 in AC_4 , or AC_1 in AC_6 .

Thus the spherical triangle $C_2D_5O_1$ (fig. 31*, Plate IV.*) has marked in it the indices $m, 1, n$, which indicates that the face $C_2d_5O_1$ of the six-faced octahedron (fig. 3, Plate I.) cuts the axis AC_3 produced at the distance $m \times AC_3$, the axis AC_2 at the point C_2 , and the third axis AC_1 produced, at $n \times AC_1$.

Again the indices $\bar{n} 1 \bar{m}$, in the triangle $C_2O_8D_9$ (fig. 31*, Plate IV.*), show that the face $C_2O_8d_9$ of the six-faced octahedron (fig. 3, Plate I.) cuts the axis AC_5 produced at a distance $n \times AC_5$, the axis AC_2 at the point C_2 , and the axis AC_6 at a distance $m \times AC_6$.

97. The indices marked on (figs. 31* and 32*, Plate IV.*), enable us readily to find the notation for any face of any form in Plate II.

In (fig. 31*, Plate IV.*) the indices $m 1 \bar{n}$ in triangle $C_2O_8d_5$

signify that the face of the six-faced octahedron marked $C_2o_5d_5$ (fig. 10, Plate II.) cuts the axis AC_3 at a distance m from A , the axis AC_2 at C_2 , and AC_6 at a distance n from A .

The indices $m \ 1 \ n$ in the triangle $C_2O_1D_5$ indicate that the face of the six-faced octahedron marked $C_2o_1d_5$, fig. 10, Plate II., cuts AC_3 at a distance m , AC_2 at C_2 , and AC_6 at a distance n from A .

98. Hence n without any sign over it signifies that the face of the six-faced octahedron which it indicates cuts the cubic axis C_1AC_6 in the direction of AC_1 produced; if it has the sign $-$ placed over it, it signifies that the face cuts the axis in the direction of AC_6 produced.

Now if m be infinite, represented by the symbol ∞ , or $\frac{1}{0}$, this signifies that the face cuts the axis neither in the direction AC_1 nor AC_6 , and that if produced ever so far in either direction it will not cut the axis C_1AC_6 , and is therefore parallel to it. Hence when $m=\infty$, \bar{m} and m indicate that the face is parallel to the axis, to AC_3 if m is in the first place, to AC_2 if in the second, and to AC_1 if in the third place.

99. Now, if in the triangle $C_2D_5O_5$ (fig. 31*, Plate IV.*), whose indices are $m \ 1 \ \bar{n}$, we make both m and n infinite, since ∞ and ∞ are the same, we see that $\infty \ 1 \ \infty$ is the index of the face $O_1O_4O_3O_5$ of the cube (fig. 1, Plate I.); also that, substituting the sign ∞ for both m and \bar{n} , the same notation $\infty \ 1 \ \infty$ stands for each of the eight triangles $C_2O_1D_5$, $C_2O_1D_1$, $C_2O_4D_1$, $C_2O_4D_8$, $C_2O_8D_8$, $C_2O_8D_9$, $C_2O_5D_9$, and $C_2O_5D_5$.

100. When n alone is infinite in the index $m \ 1 \ \bar{n}$, $m \ 1 \ \infty$ is the index of both $C_2o_5d_5$ and $C_2o_1d_5$, or of the face $C_2o_1o_5$ of the four-faced cube (fig. 9, Plate II.).

101. When $n=\infty$, and $m=1$, the index $m \ 1 \ \bar{n}$ becomes $1 \ 1 \ \infty$, which is the symbol for the four triangles $C_2d_5o_5$, $C_3d_5o_5$, $C_2o_1d_5$, and $C_3o_1d_5$, or of the face $C_2o_1C_3o_5$ of the rhombic dodecahedron (fig. 12, Plate II.).

102. When $n=m$, the index $m \ 1 \ \bar{n}$ becomes $m \ 1 \ \bar{m}$, which is that of the two triangles $C_2o_5d_5$ and $C_2d_9o_5$, or of the face $C_2d_9o_5d_5$ of the twenty-four faced trapezohedron (fig. 11, Plate II.).

103. When $m=1$, the index $m \ 1 \ \bar{n}$ becomes $1 \ 1 \ \bar{n}$, which is that of the two triangles $C_2o_5d_5$, $C_3o_5d_5$, or of the face $C_2o_5C_3$ of the three-faced octahedron (fig. 13, Plate II.).

104. When $m=1$ and $n=1$, the index $m \ 1 \ \bar{n}$ becomes $1 \ 1 \ \bar{1}$, which is the same for the six triangles, $C_2o_5d_5$, $C_3o_5d_5$, $C_3o_5d_{10}$, $C_6o_5d_{10}$, $C_6o_5d_9$, and $C_2o_5d_9$, or of the face $C_2C_3C_6$ of the octahedron (fig. 14, Plate II.).

105. To find the normal to a plane from the centre of the cubical axes in terms of the indices of that plane.

Let BCD (fig. 33*, Plate IV.*) be a plane cutting the three cubical axes AB , AC , and AD , in the points B , C , and D . Let $AB=a$, $AC=b$, and $AD=c$, be the three indices of this plane.

Through A draw AE perpendicular to BC in triangle ABC . Join ED .

Through A draw AF perpendicular to DE in triangle ADE .

Then AF is perpendicular to the plane ABC . Let $AF=R$, then R is the normal drawn through A to the plane whose indices are a , b , c .

Through F in triangle ADE draw FG perpendicular to AE , and in triangle ABC draw GH perpendicular to AB .

Let $AH=x$, $GH=y$, and $FG=z$, are called the rectangular co-ordinates of the point F , referred to the rectangular axes AB , AC , AD , or AX , AY , AZ (fig. 33*, Plate IV.*), is drawn in perspective. (Fig. 35*) is the triangle ACB of (fig. 33*), drawn on the plane of the paper; (fig. 34) the triangle DAE of the same figure, also on the plane of the paper.

Let angle $AEF=\beta$. Then by construction $AFG=\beta$, $DAF=\beta$, $ADF=90^\circ-\beta$, and $FAE=90^\circ-\beta$.

$$z=FG=AF \sin FAG=R \cos \beta.$$

$$\text{Also } R=AF=AD \sin ADF=c \cos \beta.$$

$$\text{Hence } z=\frac{R}{c}.$$

Again, in triangle AGF , $AG=AF \sin AFG=R \sin \beta$.

Also in triangle ABC , let $a=\text{angle } ABC$, then by construction $CAE=a$, $AGH=a$, $ECA=90-a$, and $EAB=90-a$.

$$\text{In triangle } AGH, x=AH=AG \sin AGH=AG \sin a=R \sin \beta \sin a.$$

$$\text{Also in triangle } AEB, AE=a \sin a \text{ and } \sin a=\frac{AE}{a}$$

$$\text{In triangle } AFE, R=AF=AE \sin \beta \text{ and } \sin \beta=\frac{R}{AE}$$

$$\text{But } x=R \sin \beta \sin a=R \cdot \frac{R}{AE} \cdot \frac{AE}{a}=\frac{R^2}{a}$$

Again in triangle AGH , $y=GH=AG \cos a=R \sin \beta \cos a$.

In triangle ACE ,

$$AE=AC \cos CAE=b \cos a; \text{ and } \cos a=\frac{AE}{b}$$

$$\text{But } \sin \beta=\frac{R}{AE}$$

$$\text{Hence } y=R \sin \beta \cos a=R \cdot \frac{R}{AE} \cdot \frac{AE}{b}=\frac{R^2}{b}$$

$$\text{Hence } x = \frac{R^2}{a}, \quad y = \frac{R^2}{b}, \quad \text{and } z = \frac{R^2}{c}$$

In triangle AGF , $R^2 = AF^2 = FG^2 + AG^2 = z^2 + AG^2$.

And in triangle AGH , $AG^2 = AH^2 + HG^2 = x^2 + y^2$.

$$\text{Hence } R^2 = x^2 + y^2 + z^2 = \frac{R^4}{a^2} + \frac{R^4}{b^2} + \frac{R^4}{c^2}$$

$$\text{And } R^2 = \frac{1}{\frac{1}{a^2} + \frac{1}{b^2} + \frac{1}{c^2}} \quad R = \frac{1}{\sqrt{\frac{1}{a^2} + \frac{1}{b^2} + \frac{1}{c^2}}}$$

106. In (fig. 33*, Plate IV.*), join CF and BF . Then because $AF=R$ is perpendicular to the plane BCD , AF is perpendicular to CF and BF as well as DF .

$$\text{Therefore } \cos FAD = \frac{R}{c} = \frac{\frac{1}{c}}{\sqrt{\frac{1}{a^2} + \frac{1}{b^2} + \frac{1}{c^2}}}$$

$$\text{also } \cos FAB = \frac{R}{a} = \frac{\frac{1}{a}}{\sqrt{\frac{1}{a^2} + \frac{1}{b^2} + \frac{1}{c^2}}}$$

$$\text{and } \cos FAC = \frac{R}{b} = \frac{\frac{1}{b}}{\sqrt{\frac{1}{a^2} + \frac{1}{b^2} + \frac{1}{c^2}}}$$

Where FAD , FAB , and FAC are the three angles which the normal makes with the three cubical axes which it cuts at the distances a , b , and c .

107. Given the indices of any two faces of a crystal of the cubical system, find the angle between their two normals at the centre of cube, or the supplement of the angle of inclination of these two faces over the edge of their intersection.

(In fig. 36*, Plate IV.*)

Let $AF=R$ be the normal to the plane whose indices are a , b , c .

$AF_1=R_1$ be the normal to the plane whose indices are a_1 , b_1 , c_1 .

Let $x=AH$, $y=HG$, and $z=FG$ be the rectangular co-ordinates of the point F (see § 105) referred to the rectangular axes AX , AY , AZ .

And $x_1=AH_1$, $y_1=H_1G_1$, $z_1=F_1G_1$, similar co-ordinates for the point F_1 .

Fig. 36* is drawn in perspective. Fig. 37* is the plane

F_1G_1GF of (fig. 36*) drawn on the plane of the paper. (Fig. 38*) the plane $YAHH_1G_1$ also drawn on the plane of the paper.

Join FF_1 and GG_1 . In plane FF_1G_1G draw KF parallel to GG_1 , and therefore perpendicular F_1G_1 ; also in plane GG_1H_1H draw GL parallel to HH_1 . Then $KFGG_1$ and $HGLH_1$ are rectangular parallelograms and their opposite sides are equal.

Then (fig. 37*) $FF_1^2 = F_1K^2 + KF^2 = (F_1G_1 - KG_1)^2 + G_1G^2$.
 $= (F_1G_1 - FG)^2 + G_1G^2 = (z_1 - z)^2 + G_1G^2$.

But (fig. 38*)

$$G_1G^2 = GL^2 + G_1L^2 = HH_1^2 + (G_1H_1 - LH_1)^2 \\ = (AH_1 - AH)^2 + (G_1H_1 - GH)^2 = (x_1 - x)^2 + (y_1 - y)^2.$$

And $FF_1^2 = (x_1 - x)^2 + (y_1 - y)^2 + (z_1 - z)^2$.

We have seen (§ 105) that $R^2 = x^2 + y^2 + z^2$, and that

$$x = \frac{R^2}{a}, \quad y = \frac{R^2}{b}, \quad \text{and} \quad z = \frac{R^2}{c}$$

Similarly $R_1^2 = x_1^2 + y_1^2 + z_1^2$, and $x_1 = \frac{R_1^2}{a}$, $y_1 = \frac{R_1^2}{b}$, and $z_1 = \frac{R_1^2}{c}$

In triangle FF_1A , fig. 39, if we put θ for the angle FAF_1 or the angle between the normals AF , AF_1 , or R and R_1 at the point A ; we have

$$FF_1^2 = AF_1^2 + AF^2 - 2AF_1 \cdot AF \cos \theta = R_1^2 + R^2 - 2RR_1 \cos \theta;$$

$$\text{but } FF_1^2 = (x_1 - x)^2 + (y_1 - y)^2 + (z_1 - z)^2.$$

$$\text{Hence } (x_1 - x)^2 + (y_1 - y)^2 + (z_1 - z)^2 = R_1^2 + R^2 - 2RR_1 \cos \theta;$$

$$\text{or } x_1^2 - 2xx_1 + x^2 + y_1^2 - 2yy_1 + y^2 + z_1^2 - 2z_1z + z^2 = R_1^2 + R^2 - 2RR_1 \cos \theta.$$

$$\text{But } R_1^2 = x_1^2 + y_1^2 + z_1^2 \quad \text{and} \quad R^2 = x^2 + y^2 + z^2.$$

$$\text{Hence } x_1x + y_1y + z_1z = RR_1 \cos \theta,$$

$$\text{or } \frac{R^2 R_1^2}{aa_1} + \frac{R^2 R_1^2}{bb_1} + \frac{R^2 R_1^2}{cc_1} = RR_1 \cos \theta.$$

$$\cos \theta = RR_1 \left(\frac{1}{aa_1} + \frac{1}{bb_1} + \frac{1}{cc_1} \right),$$

$$\text{but } R^2 = x^2 + y^2 + z^2 = \frac{R^4}{a^2} + \frac{R^4}{b^2} + \frac{R^4}{c^2} \quad \text{and} \quad R^2 = \frac{1}{\frac{1}{a^2} + \frac{1}{b^2} + \frac{1}{c^2}}$$

$$\text{also } R_1^2 = \frac{1}{\frac{1}{a_1^2} + \frac{1}{b_1^2} + \frac{1}{c_1^2}}$$

$$\text{Therefore } \cos \theta = \frac{\frac{1}{aa_1} + \frac{1}{bb_1} + \frac{1}{cc_1}}{\sqrt{\left(\frac{1}{a^2} + \frac{1}{b^2} + \frac{1}{c^2} \right) \left(\frac{1}{a_1^2} + \frac{1}{b_1^2} + \frac{1}{c_1^2} \right)}}$$

108. In fig. 33, Plate IV.*, let p_1 =angle FAB , which the normal AF makes with the axis AX ; p_2 =angle FAC , which the normal makes with the axis AY ; and p_3 =angle FAD makes with AZ .

AX is the normal to a face of the cube which cuts the axis AX at a , AY at ∞ , and AZ at ∞ ; or $a_1=a$, $b_1=\infty=\frac{1}{0}$, and $c_1=\infty=\frac{1}{0}$

$$\text{and } \cos p_1 = \frac{\frac{1}{a^2}}{\sqrt{\left(\frac{1}{a^2} + \frac{1}{b^2} + \frac{1}{c^2}\right) \frac{1}{a^2}}} = \frac{\frac{1}{a}}{\sqrt{\frac{1}{a^2} + \frac{1}{b^2} + \frac{1}{c^2}}}$$

AY is the normal to a face of the cube, or a plane whose indices are $a_1=\frac{1}{0}$, $b_1=b$, and $c_1=\frac{1}{0}$

$$\cos p_2 = \frac{\frac{1}{b}}{\sqrt{\frac{1}{a^2} + \frac{1}{b^2} + \frac{1}{c^2}}}$$

AZ is the normal to a plane whose indices are $a_1=\frac{1}{0}$, $b_1=\frac{1}{0}$, and $c_1=c$,

$$\text{and } \cos p_3 = \frac{\frac{1}{c}}{\sqrt{\frac{1}{a^2} + \frac{1}{b^2} + \frac{1}{c^2}}}$$

The same formulæ we obtained in § 106.

109. If p_1, p_2, p_3 be the angles which the normal to the plane whose indices are $a b c$, makes with the three axes AX, AY , and AZ ;

Also, q_1, q_2, q_3 the angles the normal to the plane whose indices are $a_1 b_1 c_1$, makes with the same axes,

$$\begin{aligned} \text{Then } \cos p_1 &= \frac{\frac{1}{a}}{\sqrt{\frac{1}{a^2} + \frac{1}{b^2} + \frac{1}{c^2}}} & \cos p_2 &= \frac{\frac{1}{b}}{\sqrt{\frac{1}{a^2} + \frac{1}{b^2} + \frac{1}{c^2}}} \\ & & \text{and } \cos p_3 &= \frac{\frac{1}{c}}{\sqrt{\frac{1}{a^2} + \frac{1}{b^2} + \frac{1}{c^2}}}; \end{aligned}$$

$$\text{and } \cos q_1 = \frac{\frac{1}{a_1}}{\sqrt{\frac{1}{a_1^2} + \frac{1}{b_1^2} + \frac{1}{c_1^2}}} \quad \cos q_2 = \frac{\frac{1}{b_1}}{\sqrt{\frac{1}{a_1^2} + \frac{1}{b_1^2} + \frac{1}{c_1^2}}}$$

$$\text{and } \cos q_3 = \frac{\frac{1}{c_1}}{\sqrt{\frac{1}{a_1^2} + \frac{1}{b_1^2} + \frac{1}{c_1^2}}}$$

Substituting these values in the expression

$$\cos \theta = \frac{\frac{1}{aa_1} + \frac{1}{bb_1} + \frac{1}{cc_1}}{\sqrt{\left(\frac{1}{a^2} + \frac{1}{b^2} + \frac{1}{c^2}\right) \left(\frac{1}{a_1^2} + \frac{1}{b_1^2} + \frac{1}{c_1^2}\right)}}$$

we have

$$\cos \theta = \cos p_1 \cos q_1 + \cos p_2 \cos q_2 + \cos p_3 \cos q_3.$$

110. If, in (figs. 31* and 32*), we substitute for 1, m , n ; $\cos p_1$, $\cos p_2$, and $\cos p_3$ in the order in which they occur, we have a notation for every face of the six-faced octahedron in terms of p_1 , p_2 , and p_3 , the polar distances of the face from the three adjacent poles of the cube; -1 , $-m$, and $-n$ being replaced by $-\cos p_1$, $-\cos p_2$, and $-\cos p_3$.

Thus if θ be the angle between the normals of the faces whose poles lie in the spherical triangles $C_1D_1O_1$ and $C_2O_1D_2$, or the supplement of the angle of their inclination over the edge C_1O_1 (fig. 3, Plate I.),

$$\cos \theta = \frac{\frac{1}{mn} + \frac{1}{mn} + 1}{\sqrt{\left(\frac{1}{n^2} + \frac{1}{m^2} + 1\right) \left(\frac{1}{m^2} + \frac{1}{n^2} + 1\right)}} = \frac{\frac{2}{mn} + 1}{1 + \frac{1}{m^2} + \frac{1}{n^2}}$$

if expressed by the indices of the six-faced octahedron.

$$\begin{aligned} \cos \theta &= \cos p_3 \cos p_2 + \cos p_2 \cos p_3 + \cos p_1 \cos p_1 \\ &= 2 \cos p_3 \cos p_2 + \cos^2 p_1 \end{aligned}$$

if expressed by the three polar distances of the pole of any face from the three adjacent poles of the cube.

111. The notation for each face of a crystal, or of its pole on the sphere of projection, is expressed in the terms of the three indices at which a plane drawn through a point in one of the cubical axes, taken at an arbitrary distance called unity from the centre where the axes meet, cuts the other two axes which are at right angles to the former; the indices being reckoned positive or negative as the points of intersection are right or left of A along the three axes AC_1 , AC_2 , and AC_3 .

112. The relations of any pole to any other pole, and other problems relating to crystals, can therefore be solved by that branch of Geometry of Three dimensions which relates to the properties of the plane and straight line. This method is used by Professor Naumann, of Freiberg, in his works on crystallography.

113. The use of the sphere of projection has led to that of spherical trigonometry for solving all questions of crystallography, retaining, however, the notation for the faces of crystals in terms of the indices of the plane cutting the axes derived from the geometry of the plane. Professor Miller, of Cambridge, uses Spherical Trigonometry in his works on crystallography.

114. The position of any pole on the sphere of projection may be determined by its polar distance from a definite pole on the sphere corresponding to the north pole of the terrestrial sphere, and its longitude by an arc measured along the equator of the fixed pole, from a definite point in that equator. Just as the position of any point on the earth's surface is determined by its latitude and longitude.

In the crystallographic sphere of projection it is more convenient to use the polar distance instead of the latitude; the polar distance being an arc 90° less than that of the latitude.

115. The forms of the cubical system possess the highest degree of symmetry, each face of every form being symmetrical right and left from the centre to each of the three cubical axes. Hence we have seen that the three indices taken positive or negative, or right and left of the centre, give the notation or express this degree of symmetry.

116. In (figs. 31* and 32*, Plate IV.*), we see that if in the sphere of projection we take C_1 as the north pole and C_6 as the south, and $C_3C_2C_5C_4$ as the equator, and measure longitude from C_3 .

If p be the north polar distance of the face $1\ m\ n$ and λ be its longitude,

Then p will be the north polar distance of the eight faces or poles $1\ m\ n$, $m\ 1\ n$, $\bar{m}\ 1\ n$, $\bar{1}\ m\ n$, $\bar{1}\ \bar{m}\ n$, $\bar{m}\ \bar{1}\ n$, $m\ \bar{1}\ n$, and $1\ \bar{m}\ n$, whose longitudes are λ , $90 - \lambda$, $90 + \lambda$, $180 - \lambda$, $180 + \lambda$, $270 - \lambda$, $270 + \lambda$, and $360 - \lambda$.

Also p will be the south polar distance of the eight faces $1\ m\ \bar{n}$, $m\ 1\ \bar{n}$, $\bar{m}\ 1\ \bar{n}$, $\bar{1}\ m\ \bar{n}$, $\bar{1}\ \bar{m}\ \bar{n}$, $\bar{m}\ \bar{1}\ \bar{n}$, $m\ \bar{1}\ \bar{n}$, and $1\ m\ \bar{n}$, whose longitudes are respectively the same as the former.

Again, if we take C_2 as the north pole, C_4 as the south, and $C_1C_3C_6C_5$ as the equator, and measure the longitude from C_1 , we have eight faces, $m\ n\ 1$, $1\ n\ m$, $1\ n\ \bar{m}$, $m\ n\ \bar{1}$, $\bar{m}\ \bar{n}\ 1$, $\bar{1}\ n\ \bar{m}$, $1\ n\ \bar{m}$, and $\bar{m}\ n\ 1$, having the same north polar distances and the same longitudes as the former.

Also eight more faces $m \bar{n} 1$, $1 \bar{n} m$, $1 \bar{n} \bar{m}$, $m \bar{n} \bar{1}$, $\bar{m} \bar{n} \bar{1}$, $\bar{1} \bar{n} \bar{m}$, $\bar{1} \bar{n} m$, $\bar{m} \bar{n} 1$, having the same south polar distance and longitudes as the former.

The 16 other faces will have the similar polar distances and longitudes, taking C_3 as the north and C_5 as the south pole, and $C_1 C_2 C_6 C_4$ as the equator.

117. (Fig. 39*, Plate IV.*).—In the three rectangular cubical axes, take $AB=1$, $AM=m$, $AN=n$.

Through A draw AG perpendicular MB , AH perpendicular NC_3 , and AK perpendicular MN .

Join NG , HM , and BK meeting in F . Join AF .

Since the normal from A or the perpendicular to the plane NMB must, by construction, lie in each of the three planes NAG , HAM , and KAB , AF , their common intersection, must be the normal to the plane NMB .

Hence AF is the normal to the plane whose notation is $1 m n$. AG is the normal to a plane passing through MC_3 parallel to AN , or the normal to a face of the four-faced cube whose notation is $1 m \infty$, AH the normal to $1 \infty n$, AK to $\infty m n$ or $\infty 1 \frac{n}{m}$.

(Fig. 40*, Plate IV.*).—Let C_1 , C_2 , C_3 be the poles of the three rectangular or cubical axes, or the points where AN , AM , and AB of fig. 39* cut the sphere of projection.

Let h , k , and g be the points where AH , AK , and AG cut the sphere of projection. Join $C_1 g$, $C_3 k$, and $C_2 h$ by arcs of great circles meeting in f .

Then g is the pole of $1 m \infty$, h of $1 \infty n$, k of $\infty 1 \frac{n}{m}$, and f of $1 m n$.

Let $fC_3=p_1$, $fC_2=p_2$, $fC_1=p_3$, $C_2 k=\lambda_1$, $C_3 h=\lambda_2$, $C_3 g=\lambda_3$.

Then p_1 , p_2 , and p_3 will be the polar distances of the pole of $1 m n$ from C_3 , C_2 , and C_1 , taken in order of magnitude.

Comparing § 96 with (fig. 31*, Plate IV.*), the face $1 m n$ cuts the axis AC_3 in B , AC_2 in M , and AC_1 in N to form (fig. 39*). Hence arc $C_1 f$ (fig. 40*) $=p_3$, and $C_3 g=\lambda_3$, is its polar distance and longitude.

The face $1 n m$ cuts the axis AC_3 in B , AC_2 in N , and AC_1 in M ; and (fig. 40*) $C_2 f=p_2$ and $C_3 h=\lambda_2$, is its polar distance and longitude.

Also the face $m n 1$ cuts the axis AC_3 in M , AC_2 in N , and AC_1 in B ; and (fig. 40*) $C_3 f=p_1$ and $C_2 k=\lambda_1$, is its polar distance and longitude.

Calling (figs. 31* and 32*, Plate IV.*), C_1 the North pole, $C_3 C_2 C_5$ the equator, and measuring longitude from C_3 , λ_3 will be the longitude of $1 m n$, $90^\circ - \lambda_3$ of $m 1 n$, $90^\circ + \lambda_3$ of $\bar{m} 1 n$,

$180^\circ - \lambda_3$ of $\bar{1} m n$, $180^\circ + \lambda_3$ of $\bar{1} \bar{m} n$, $270^\circ - \lambda_3$ of $\bar{m} \bar{1} n$, $270^\circ + \lambda_3$ of $m \bar{1} n$, and $360^\circ - \lambda_3$ of $1 \bar{m} n$.

The north polar distances of these eight faces will each be p_3 .

λ_3 the longitude of $1 m \bar{n}$, $90^\circ - \lambda_3$ of $m 1 \bar{n}$, $90^\circ + \lambda_3$ of $\bar{m} 1 n$, $180^\circ - \lambda_3$ of $\bar{1} m \bar{n}$, $180^\circ + \lambda_3$ of $\bar{1} \bar{m} \bar{n}$, $270^\circ - \lambda_3$ of $m \bar{1} \bar{n}$, $270^\circ + \lambda_3$ of $m \bar{1} \bar{n}$, and $360^\circ - \lambda_3$ of $1 \bar{m} \bar{n}$.

The north polar distances of these eight faces will each be $180^\circ - p_3$.

λ_2 will be the longitude of $1 n m$, $90^\circ - \lambda_2$ of $n 1 m$, $90^\circ + \lambda_2$ of $\bar{n} 1 m$, $180^\circ - \lambda_2$ of $\bar{1} n m$, $180^\circ + \lambda_2$ of $\bar{1} \bar{n} m$, $270^\circ - \lambda_2$ of $\bar{n} \bar{1} m$, $270^\circ + \lambda_2$ of $n \bar{1} m$, $360^\circ - \lambda_2$ of $1 \bar{n} m$.

The north polar distances of these eight faces will each be p_2 .

The eight similar faces in the southern hemisphere will have the same longitudes as those corresponding to them in the northern, the eight north polar distances being each equal $180^\circ - p_2$.

λ_1 will be the longitude of $m n 1$, $90^\circ - \lambda_1$ of $n m 1$, $90^\circ + \lambda_1$ of $\bar{n} m 1$, $180^\circ - \lambda_1$ of $\bar{m} n 1$, $180^\circ + \lambda_1$ of $\bar{m} \bar{n} 1$, $270^\circ - \lambda_1$ of $\bar{n} \bar{m} 1$, $270^\circ + \lambda_1$ of $n \bar{m} 1$, and $360^\circ - \lambda_1$ of $m \bar{n} 1$.

p_1 will be the north polar distance of each of these eight faces.

The corresponding eight faces of the southern hemisphere will have the same longitudes as the corresponding ones in the northern, $180^\circ - p_1$ being the north polar distance of these eight faces.

Hence the 48 faces or poles of the six-faced octahedron can be expressed in terms of p_1 , λ_1 , p_2 , λ_2 , and p_3 , λ_3 ; and, as all other forms of the cubical system can be derived from those of the six-faced octahedron, all faces of those forms can be similarly expressed.

118. Given p_3 and λ_3 to determine p_1 and λ_1 , and also p_2 and λ_2 in terms of the former.

From the spherical triangle $C_1 f C_3$ (fig. 40*, Plate IV.*), we have by the formulæ of spherical trigonometry,

$\cos f C_3 = \cos C_1 C_3 \cos C_1 f + \sin C_1 C_3 \sin C_1 f \cos f C_1 C_3$;
but the spherical angle $f C_1 C_3$ is measured by the arc $g C_3$ at the equator.

Hence, substituting the values of these arcs given in the previous section, we have

$$\begin{aligned} \cos p_1 &= \cos 90^\circ \cos p_3 + \sin 90^\circ \sin p_3 \cos \lambda_3 \\ &= \sin p_3 \cos \lambda_3. \end{aligned}$$

Again, in the spherical triangle $f g C_3$, we have

$$\frac{\sin f g}{\sin f C_3} = \frac{\sin f C_3 g}{\sin f g C_3};$$

but spherical triangle fC_3g is measured by arc kC_2 , and fgC_3 is 90° ; hence

$$\frac{\sin (90^\circ - p_3)}{\sin p_1} = \frac{\sin \lambda_1}{\sin 90^\circ} \quad \text{and} \quad \sin \lambda_1 = \frac{\cos p_3}{\sin p_1}$$

From the spherical triangle C_1C_2f , we have

$$\begin{aligned} \cos C_2f &= \cos C_1C_2 \cos C_1f + \sin C_1C_2 \sin C_1f \cos C_2C_1f, \\ \text{or, } \cos p_2 &= \cos 90^\circ \cos p_3 + \sin 90^\circ \sin p_3 \cos (90^\circ - \lambda_3) \\ &= \sin p_3 \sin \lambda_3. \end{aligned}$$

From the spherical triangle C_2fg , we have

$$\frac{\sin C_2f}{\sin fg} = \frac{\sin C_2gf}{\sin fC_2g} \quad \text{or} \quad \frac{\sin p_2}{\sin 90^\circ - p_3} = \frac{\sin 90^\circ}{\sin \lambda_2}$$

$$\text{and } \sin \lambda_2 = \frac{\cos p_3}{\sin p_2}$$

$$\text{Hence } \cos p_1 = \sin p_3 \cos \lambda_3, \quad \sin \lambda_1 = \frac{\cos p_3}{\sin p_1};$$

$$\text{and } \cos p_2 = \sin p_3 \sin \lambda_3, \quad \sin \lambda_2 = \frac{\cos p_3}{\sin p_2}$$

119. To find the angle between the poles of two faces in terms of their polar distances and longitudes.

Let C_1F be the polar distance of F (fig. 41, Plate IV.*), C_3L its longitude, C_1f the polar distance, and C_3l the longitude of f .

Also let $C_1F = P_3$, $C_1f = p_3$; $C_3L = L_3$, $C_3l = \lambda_3$, and $Ff = \theta$.

Then in spherical triangle C_1Ff

$$\cos Ff = \cos C_1F \cos C_1f + \sin C_1F \sin C_1f \sin FC_1f.$$

Then angle FC_1f is measured by arc $Ll = LC_3 - lC_3$.

Hence $\cos \theta = \cos P_3 \cos p_3 + \sin P_3 \sin p_3 \cos (L_3 - \lambda_3)$.

To adapt this to logarithmic computation—

$$\cos \theta = \cos p_3 \{ \cos P_3 + \sin P_3 \tan p_3 \cos (L_3 - \lambda_3) \}.$$

Let $\tan a = \tan p_3 \cos (L_3 - \lambda_3)$.

Then $\cos \theta = \cos p_3 \{ \cos P_3 + \tan a \cdot \sin P_3 \}$

$$= \frac{\cos p_3}{\cos a} \{ \cos P_3 \cos a + \sin P_3 \sin a \}$$

$$= \frac{\cos p_3}{\cos a} \cos (P_3 - a).$$

120. To find the distance between any two poles on the sphere of projection in terms of the three polar distances from C_1 , C_2 , and C_3 .

$$\begin{aligned} \S 119. \quad \cos \theta &= \cos P_3 \cos p_3 + \sin P_3 \sin p_3 \cos (L_3 - \lambda_3) \\ &= \cos P_3 \cos p_3 + \sin P_3 \sin p_3 (\cos L_3 \cos \lambda_3 \\ &\quad + \sin L_3 \sin \lambda_3) \\ &= \cos P_3 \cos p_3 + \sin P_3 \sin p_3 \cos L_3 \cos \lambda_3 \\ &\quad + \sin P_3 \sin p_3 \sin L_3 \sin \lambda_3; \end{aligned}$$

$$\begin{aligned} \text{but } \S 118, \quad \cos p_1 &= \sin p_3 \cos \lambda_3 & \cos P_1 &= \sin P_3 \cos L_3 \\ \cos p_2 &= \sin p_3 \sin \lambda_3 & \cos P_2 &= \sin P_3 \sin L_3. \end{aligned}$$

Hence $\cos \theta = \cos P_3 \cos p_3 + \cos P_2 \cos p_2 + \cos P_1 \cos p_1$.

The same formulæ which we obtained by geometry of three dimensions, § 109.

121. To find the polar distances and longitudes in terms of the indices.

Referring to § 117 and (fig. 40*, Plate IV.*), C_1 , C_2 , and C_3 are poles of the cube, f is a pole of $1\ m\ n$, g of $1\ m\ \infty$, h of $1\ \infty\ n$, k of $\infty\ 1\ \frac{n}{m}$, C_3 of $1\ \infty\ \infty$, C_2 of $\infty\ 1\ \infty$, and C_1 of $\infty\ \infty\ 1$.
 $fC_3 = p_1$, $fC_2 = p_2$, $fC_1 = p_3$, $C_2k = \lambda_1$, $C_3h = \lambda_2$, $C_3g = \lambda_3$.

Then λ_1 is the distance between the poles of $\infty\ 1\ \frac{n}{m}$ and $\infty\ 1\ \infty$, p_1 that between $1\ m\ n$ and $1\ \infty\ \infty$.

Hence, § 107,

$$\begin{aligned}\cos \lambda_1 &= \frac{1}{\sqrt{1 + \frac{m^2}{n^2}}} & \cos p_1 &= \frac{1}{\sqrt{1 + \frac{1}{m^2} + \frac{1}{n^2}}} \\ \sec^2 \lambda_1 &= 1 + \frac{m^2}{n^2} & \sec^2 p_1 &= 1 + \frac{1}{m^2} + \frac{1}{n^2} \\ \tan^2 \lambda_1 &= \frac{m^2}{n^2} & \tan^2 p_1 &= \frac{1}{m^2} + \frac{1}{n^2} = \frac{1}{m^2} \left(1 + \frac{m^2}{n^2}\right) \\ \tan \lambda_1 &= \frac{m}{n} & &= \frac{1}{m} \sec^2 \lambda_1 \\ n &= m \cot \lambda_1 & \tan p_1 &= \frac{1}{m} \sec \lambda_1 \\ &= \cot \lambda_1 \sec \lambda_1 \cot p_1 & m &= \sec \lambda_1 \cot p_1 \\ &= \frac{\cot p_1}{\sin \lambda_1}\end{aligned}$$

Again λ_2 is the distance between $1\ \infty\ n$ and $1\ \infty\ \infty$, p_2 that between $1\ m\ n$ and $\infty\ 1\ \infty$.

Hence, § 107,

$$\begin{aligned}\cos \lambda_2 &= \frac{1}{\sqrt{1 + \frac{1}{n^2}}} & \cos p_2 &= \frac{1}{m} \frac{1}{\sqrt{1 + \frac{1}{m^2} + \frac{1}{n^2}}} \\ \sec^2 \lambda_2 &= 1 + \frac{1}{n^2} & \sec^2 p_2 &= m^2 \left(1 + \frac{1}{m^2} + \frac{1}{n^2}\right) \\ \tan^2 \lambda_2 &= \frac{1}{n^2} & &= 1 + m^2 \left(1 + \frac{1}{n^2}\right) \\ n &= \cot \lambda_2 & \tan^2 p_2 &= m^2 \sec^2 \lambda_2 \\ & & \tan p_2 &= m \sec \lambda_2 \\ & & m &= \tan p_2 \cos \lambda_2\end{aligned}$$

Also λ_3 is the distance between $1\ m\ \infty$ and $1\ \infty\ \infty$, p_3 that between $1\ m\ n$ and $\infty\ \infty\ 1$.

And, § 107,

$$\begin{aligned}\cos \lambda_3 &= \frac{1}{\sqrt{1 + \frac{1}{m^2}}} & \cos p_3 &= \frac{\frac{1}{n}}{\sqrt{1 + \frac{1}{m^2} + \frac{1}{n^2}}} \\ \sec^2 \lambda_3 &= 1 + \frac{1}{m^2} & \sec^2 p_3 &= n^2 \left(1 + \frac{1}{m^2} + \frac{1}{n^2} \right) \\ \tan^2 \lambda_3 &= \frac{1}{m^2} & &= 1 + n^2 \left(1 + \frac{1}{m^2} \right) \\ m &= \cot \lambda_3 & \tan^2 p_3 &= n^2 \sec^2 \lambda_3 \\ & & n &= \tan p_3 \cos \lambda_3.\end{aligned}$$

Hence the indices being given, the polar distances and longitudes can be determined, or the polar distances and longitudes being given the indices can be determined.

122. To find the polar distances of any two adjacent poles of faces of the six-faced octahedron, or of the supplement of the angle over the edge of any two adjacent faces, in terms of the indices.

Let θ be the angle between any two poles adjacent to the arc CO (figs. 31* and 32*, Plate IV.*), ϕ adjacent to OD , and ψ adjacent to CD .

For the faces $n m 1$, $m n 1$,

$$\cos \theta = \frac{\frac{1}{mn} + \frac{1}{mn} + 1}{\sqrt{\left(\frac{1}{n^2} + \frac{1}{m^2} + 1\right)\left(\frac{1}{m^2} + \frac{1}{n^2} + 1\right)}} = \frac{\frac{2}{mn} + 1}{1 + \frac{1}{m^2} + \frac{1}{n^2}}$$

Similarly for $1 n m$ and $1 m n$ we have

$$\cos \theta = \frac{\frac{2}{mn} + 1}{1 + \frac{1}{m^2} + \frac{1}{n^2}}$$

The same is true over every arc CO in (figs. 31* and 32*, Plate IV.*).

$$\text{For the faces } m 1 n \text{ and } 1 m n \cos \phi = \frac{\frac{1}{m} + \frac{1}{m} + \frac{1}{n^2}}{1 + \frac{1}{m^2} + \frac{1}{n^2}} = \frac{\frac{2}{m} + \frac{1}{n^2}}{1 + \frac{1}{m^2} + \frac{1}{n^2}}$$

$$\text{For the faces } m 1 n, m 1 \bar{n} \cos \psi = \frac{\frac{1}{m^2} + 1 - \frac{1}{n^2}}{1 + \frac{1}{m^2} + \frac{1}{n^2}}$$

123. To express θ , ϕ , and ψ in terms of the polar distances and longitudes.

Then, according to § 110, if we substitute $\cos p_1$ for 1, $\cos p_2$ for m , and $\cos p_3$ for n ,

We have for the faces $n m l$ and $m n l$, or

$$\begin{aligned} & \cos p_3, \cos p_2, \cos p_1, \text{ and } \cos p_2, \cos p_3, \cos p_1. \\ \text{and } \cos \theta &= \cos p_2 \cos p_3 + \cos p_2 \cos p_3 + \cos^2 p_1 \\ &= 2 \cos p_2 \cos p_3 + \cos^2 p_1. \end{aligned}$$

For the faces $m l n$, and $l m n$, or

$$\begin{aligned} & \cos p_2, \cos p_1, \cos p_3, \text{ and } \cos p_1, \cos p_2, \cos p_3, \\ \text{and } \cos \phi &= \cos p_1 \cos p_2 + \cos p_1 \cos p_2 + \cos^2 p_3 \\ &= 2 \cos p_1 \cos p_2 + \cos^2 p_3. \end{aligned}$$

Also for the faces $m l n$ and $m l \bar{n}$, or

$$\begin{aligned} & \cos p_2, \cos p_1, \cos p_3, \text{ and } \cos p_2, \cos p_1, \cos p_3, \\ \cos \psi &= \cos^2 p_2 + \cos^2 p_1 - \cos^2 p_3. \end{aligned}$$

But referring to § 118 $\cos^2 p_2 = \sin^2 p_3 \sin^2 \lambda_3$

$$\text{and } \cos^2 p_1 = \sin^2 p_3 \cos^2 \lambda_2.$$

$$\begin{aligned} \text{Hence } \cos \psi &= \sin^2 p_3 \sin^2 \lambda_3 + \sin^2 p_3 \cos^2 \lambda_3 - \cos^2 p_3 \\ &= \sin^2 p_3 - \cos^2 p_3 = 2 \sin^2 p_3 - 1. \end{aligned}$$

$$\text{And } 1 + \cos \psi = 2 \sin^2 p_3.$$

$$\text{Therefore } 2 \cos^2 \frac{\psi}{2} = 2 \sin^2 p_3,$$

$$\text{and } \cos \frac{\psi}{2} = \sin p_3 = \cos (90^\circ - p_3).$$

$$\text{Whence } \frac{\psi}{2} = 90^\circ - p_3, \text{ or } \psi = 180^\circ - 2 p_3.$$

This result might have been obtained at once by inspection from (fig. 31*, Plate IV.*) For p_3 is the north polar distance of the face $l m n$, and $180^\circ - p_3$ that of $l m \bar{n}$. The poles of both these faces also lie in the same meridian.

$$\text{Hence } \phi = 180^\circ - p_3 - p_3 = 180^\circ - 2 p_3.$$

Again, using the formulæ § 119, θ is the inclination of the pole of the face $m n l$ to that of $n m l$, p_1 the north polar distance of the pole of $m n l$, and λ_1 its longitude referred to C_1 as north pole, and $C_3 C_2 C_5$ as equator and measured from C_3 . p_1 the north polar distance of $n m l$ and $90 - \lambda_1$ its longitude referred to the same north pole and equator.

$$\begin{aligned} \text{Hence } \cos \theta &= \cos p_1 \cos p_1 + \sin p_1 \sin p_1 \cos (90 - 2 \lambda_1) \\ &= \cos^2 p_1 + \sin^2 p_1 \cos (90 - 2 \lambda_1) \\ &= 1 - \sin^2 p_1 + \sin^2 p_1 \cos (90 - 2 \lambda_1); \\ \text{and } 1 - \cos \theta &= \sin^2 p_1 \{1 - \cos (90 - 2 \lambda_1)\}. \end{aligned}$$

$$\text{Therefore } 2 \sin^2 \frac{\theta}{2} = 2 \sin^2 p_1 \sin^2 \frac{90 - 2 \lambda_1}{2}$$

$$\text{and } \sin \frac{\theta}{2} = \sin p_1 \sin (45 - \lambda_1).$$

In like manner, since p_3 and λ_3 ; and p_3 and $90 - \lambda_3$, are the polar distances and longitudes of the faces 1 m n and m 1 n referred to C_1 as north pole, and $C_3C_2C_5$ as equator,

$\cos \phi = \cos p_3 \cos p_3 + \sin p_3 \sin p_3 \cos (90 - 2\lambda_3)$,
which gives as above

$$\sin \frac{\phi}{2} = \sin p_3 \sin (45 - \lambda_3).$$

124. Given ϕ and ψ , find p_3 and λ_3 .

We have seen, § 123, that $p_3 = 90 - \frac{\psi}{2}$;

$$\text{also } \sin \frac{\phi}{2} = \sin p_3 \sin (45 - \lambda_3),$$

$$\text{therefore } \sin (45 - \lambda_3) = \frac{\sin \frac{\phi}{2}}{\sin p_3}$$

125. Given ψ and θ , find p_3 and λ_3 .

$$\S 123. \quad p_3 = 90 - \frac{\psi}{2}.$$

$$\begin{aligned} \sin \frac{\theta}{2} &= \sin (45 - \lambda_1) \sin p_1. \\ &= (\sin 45 \cos \lambda_1 - \cos 45 \sin \lambda_1) \sin p_1; \end{aligned}$$

$$\text{but } \sin 45 = \cos 45 = \frac{1}{\sqrt{2}}$$

$$\therefore \sqrt{2} \sin \frac{\theta}{2} = \sin p_1 \cos \lambda_1 - \sin p_1 \sin \lambda_1.$$

Referring to (fig. 40*, Plate IV.*), and remembering from § 117, that

$$\begin{array}{lll} p_1 = fC_3 & p_2 = fC_2 & p_3 = fC_1 \\ \lambda_1 = C_2k & \lambda_2 = C_3h & \lambda_3 = C_3g. \end{array}$$

From the spherical triangle $f g C_3$, we have

$$\frac{\sin fC_3}{\sin f g C_3} = \frac{\sin f g}{\sin f C_3 g} \quad \text{or} \quad \frac{\sin p_1}{\sin 90} = \frac{\sin (90 - p_3)}{\sin \lambda_1} = \frac{\cos p_3}{\sin \lambda_1}$$

Therefore $\sin p_1 \sin \lambda_1 = \cos p_3$.

Also from spherical triangle $C_1 f C_3$, we have

$$\frac{\sin fC_3}{\sin f C_1 C_3} = \frac{\sin fC_1}{\sin f C_3 C_1} \quad \text{or} \quad \frac{\sin p_1}{\sin \lambda_3} = \frac{\sin p_3}{\sin (90 - \lambda_1)} = \frac{\sin p_3}{\cos \lambda_1}.$$

Therefore $\sin p_1 \cos \lambda_1 = \sin p_3 \sin \lambda_3$.

Hence

$$\sqrt{2} \sin \frac{\theta}{2} = \sin p_1 \cos \lambda_1 - \sin p_1 \sin \lambda_1 = \sin p_3 \sin \lambda_3 - \cos p_3$$

$$= \sin \left(90 - \frac{\psi}{2} \right) \sin \lambda_3 - \cos \left(90 - \frac{\psi}{2} \right) = \cos \frac{\psi}{2} \sin \lambda_3 - \sin \frac{\psi}{2}$$

$$\text{Hence } \cos \frac{\psi}{2} \sin \lambda_3 = \sqrt{2} \sin \frac{\theta}{2} + \sin \frac{\psi}{2} = \sec 45^\circ \sin \frac{\theta}{2} + \sin \frac{\psi}{2}.$$

$$\begin{aligned}\text{and } \sin \lambda_3 &= \frac{1}{\cos \frac{\psi}{2}} \left\{ \sec 45^\circ \sin \frac{\theta}{2} + \sin \frac{\psi}{2} \right\} \\ &= \frac{\sec 45^\circ \sin \frac{\theta}{2}}{\cos \frac{\psi}{2}} \left\{ 1 + \frac{\sin \frac{\psi}{2}}{\sec 45^\circ \sin \frac{\theta}{2}} \right\}\end{aligned}$$

$$\text{Let } \tan^2 \alpha = \frac{\sin \frac{\psi}{2} \cos 45^\circ}{\sin \frac{\theta}{2}}$$

$$\text{Then } \sin \lambda_3 = \frac{\sin \frac{\theta}{2}}{\cos 45^\circ \cos \frac{\psi}{2}} \{1 + \tan^2 \alpha\} = \frac{\sin \frac{\theta}{2}}{\cos 45^\circ \cos \frac{\psi}{2} \cos^2 \alpha}$$

126. Given ϕ and θ , find p_3 and λ_3 .

(Fig. 42*, Plate IV*.)—Let a_1 be the pole of $l m n$, a_2 that of $l n m$, and a_3 that of $m l n$.

Join a_1, a_2 by arc of great circle cutting oC_3 in f ,

and a_1, a_3 by arc of great circle cutting od in e ;

also C_1, a_1 by $C_1 a_1$ cutting dC_3 in g ,

and Oa_1 cutting dC_3 in h .

Then $C_1 a_1 = p_3$, $C_3 g = \lambda_3$, $C_1 o = 54^\circ 44'$, and $C_2 od = 60^\circ$; and let $oa_1 = P$, $C_2 oa_1 = L$.

$$\text{Also } a_3 a_1 = \phi \text{ and } ea_1 = \frac{\phi}{2} \quad a_1 a_2 = \theta \quad a_1 f = \frac{\theta}{2}$$

$$\text{From spherical triangle } oa_1 f \quad \frac{\sin a_1 f}{\sin a_1 o f} = \frac{\sin oa_1}{\sin of a_1}$$

$$\text{therefore } \frac{\sin \frac{\theta}{2}}{\sin L} = \frac{\sin P}{\sin 90^\circ}$$

$$\text{Also in spherical triangle } oa_1 e \quad \frac{\sin a_1 e}{\sin eoa_1} = \frac{\sin oa_1}{\sin oea_1}$$

$$\text{and } \frac{\sin \frac{\phi}{2}}{\sin (60^\circ - L)} = \frac{\sin P}{\sin 90^\circ}$$

$$\text{Hence } \frac{\sin \frac{\theta}{2}}{\sin L} = \frac{\sin \frac{\phi}{2}}{\sin (60^\circ - L)}$$

$$\text{and } \frac{\sin \frac{\theta}{2}}{\sin \frac{\phi}{2}} = \frac{\sin L}{\sin (60^\circ - L)} = \frac{\sin L}{\sin 60^\circ \cos L - \cos 60^\circ \sin L}$$

$$\frac{\sin \frac{\theta}{2}}{\sin \frac{\phi}{2}} = \frac{\sin L}{\frac{\sqrt{3}}{2} \cos L - \frac{1}{2} \sin L}$$

$$\frac{\sin \frac{\phi}{2}}{\sin \frac{\theta}{2}} = \frac{\sqrt{3}}{2} \cot L - \frac{1}{2}$$

$$\sqrt{3} \cot L = 1 + \frac{2 \sin \frac{\phi}{2}}{\sin \frac{\theta}{2}}$$

$$\text{Let } \tan^2 a = \frac{2 \sin \frac{\phi}{2}}{\sin \frac{\theta}{2}} = \frac{\sin \frac{\phi}{2}}{\sin 30^\circ \sin \frac{\theta}{2}}$$

$$\text{Therefore } \sqrt{3} \cot L = 1 + \tan^2 a = \frac{1}{\cos^2 a}$$

$$\text{and } \tan L = \sqrt{3} \cos^2 a = \tan 60^\circ \cos^2 a.$$

$$\text{But we have seen that } \frac{\sin P}{\sin 90^\circ} = \frac{\sin \frac{\theta}{2}}{\sin L} \text{ and } \sin P = \frac{\sin \frac{\theta}{2}}{\sin L}$$

Also from spherical triangle $C_1 o a_1$ we have

$$\begin{aligned} \cos C_1 a_1 &= \cos C_1 o \cos o a_1 + \sin C_1 o \sin o a_1 \cos C_1 o a_1; \\ \text{or } \cos p_3 &= \cos 54^\circ 44' \cos P + \sin 54^\circ 44' \sin P \cos (120^\circ + L). \\ &= \cos 54^\circ 44' \cos P - \sin 54^\circ 44' \sin P \cos (60^\circ - L). \\ &= \cos P [\cos 54^\circ 44' - \sin 54^\circ 44' \tan P \cos (60^\circ - L)]. \end{aligned}$$

$$\text{Let } \tan \beta = \tan P \cos (60^\circ - L).$$

$$\text{Therefore } \cos p_3 = \cos P \{ \cos 54^\circ 44' - \sin 54^\circ 44' \tan \beta \}$$

$$= \frac{\cos P}{\cos \beta} \{ \cos 54^\circ 44' \cos \beta - \sin 54^\circ 44' \sin \beta \}$$

$$\cos p_3 = \frac{\cos P}{\cos \beta} \cos (54^\circ 44' + \beta).$$

Also in spherical triangle $C_1 e a_1$,

$$\frac{\sin C_1 a_1}{\sin C_1 e a_1} = \frac{\sin a_1 e}{\sin e C_1 a_1} \text{ or } \frac{\sin p_3}{\sin 90^\circ} = \frac{\sin \frac{\phi}{2}}{\sin (45^\circ - \lambda_3)}$$

$$\text{and } \sin (45^\circ - \lambda_3) = \frac{\sin \frac{\phi}{2}}{\sin p_3}$$

Hence on the whole we have the formulæ

$$\begin{aligned}\tan^2 \alpha &= \frac{\sin \frac{\phi}{2}}{\sin 30^\circ \sin \frac{\theta}{2}} \\ \tan L &= \tan 60^\circ \cos^2 \alpha. \\ \sin P &= \frac{\sin \frac{\theta}{2}}{\sin L} \quad \tan \beta = \tan P \cos (60^\circ - L). \\ \cos p_3 &= \frac{\cos P}{\cos \beta} \cos (54^\circ 44' + \beta).\end{aligned}$$

$$\text{and } \sin (45^\circ - \lambda_3) = \frac{\sin \frac{\phi}{2}}{\sin p_3}$$

for determining p_3 and λ_3 in terms of ϕ and θ ; all the formulæ being adapted for logarithmic computation.

p_3 and λ_3 being determined from the values of ϕ , θ , and ψ , m and n can be expressed in terms of p_3 and λ_3 .

127. By the formulæ given in § 124, § 125, and § 126, any two of the angles of inclination such as ϕ , θ , and ψ , over the edges of a six-faced octahedron, having been observed by the goniometer, p_3 and λ_3 can be determined. Again, by formulæ in § 118, p_1 and λ_1 , p_2 and λ_2 can be obtained from the values of p_3 and λ_3 .

p_3 and λ_3 being determined, m and n can be obtained. Now all the forms of the cubical system are derived from those of the six-faced octahedron.

Hence by determining θ , ϕ , and ψ for any form of the cubical system, we can obtain the values both of p_3 and λ_3 , and also of the indices 1, m , and n .

As we advance in this treatise we shall show good reasons for preferring the polar circular co-ordinates p_3 and λ_3 to the linear ratios or fractions m and n .

128. The problems of crystallography being resolved for the most part into those of spherical trigonometry, may be solved by means of lines drawn on the surface of a solid sphere.

This being inconvenient in practice, it is usual to project the points or poles on the surface of the sphere upon those of a plane, just as geographical and astronomical maps are projections from the surface of the sphere upon the plane of the paper on which the map is drawn. There are three principal projections of the sphere,—the *stereographic*, *orthographic*, and *gnomic*.

The *stereographic* when the eye is supposed to be placed on the surface of the sphere and the points in the hemisphere furthest from the eye are projected on the plane of the equator;

considering the point of sight or projection, the pole of the great circle on which the projection is made.

In this projection the projections of circles on the sphere are either straight lines or circles.

The *orthographic* where the eye is supposed to be placed at an infinite distance from the sphere. In this projection points on the surface of the sphere are projected on the plane of the equator by perpendiculars from those points to that plane.

In this case all great circles inclined to the equator are projected into ellipses on the plane of projection.

The *gnomic* where the eye is placed in the centre of the sphere, and the plane of projection is a plane touching the surface of the sphere.

In this projection all great circles are projected into a straight line.

From the difficulty of describing arcs of ellipses the *orthographic* projection is not suited to crystallographical problems.

The *stereographic* is that mostly used by Professor Miller and other distinguished crystallographers, but there is some trouble in finding the centres of the arcs of great circles on the sphere of projection.

The most simple projection for most purposes is the *gnomic*. By either the stereographic or gnomonic projection, many problems may be very expeditiously solved by simple geometrical constructions.

129. Comparing (fig. 14, Plate II.) with (fig. 27, Plate IV.), we see that if we take A , the centre of the cube, for the centre of the sphere of projection, and AO_1 , AO_2 , &c., AO_8 as equal radii of that sphere,—the eight faces, C_1 , C_2 , C_3 , &c., of the octahedron will each be tangent planes, touching the sphere in the eight points o_1 , o_2 , &c., o_8 . Because each of these plane faces are respectively perpendicular to AO_1 , AO_2 , &c., at the points o_1 , o_2 , &c.

The projections on the faces of the octahedron will be the same as in the former case if we regard the sphere of projection as the sphere inscribed in the cube touching the cube in the points C_1 , C_2 , &c., C_6 .

All the poles, therefore, of all the forms of the cubical system can therefore be projected on to the planes of the octahedron inscribed in the cube,—one octant of the sphere upon each face. In (fig. 14, Plate II.), as shown in perspective, and (fig. 33, Plate IV.), on the plane of the paper,—the equilateral triangle $C_1C_2C_3$ represents the gnomonic projection of an octant of the sphere of projection.

$C_1C_2C_3$ being the projections of three poles of the cube.

Bisect C_1C_2 in d_1 , C_1C_3 in d_2 , and C_2C_3 in d_5 .

d_1, d_2 , and d_5 are projections of the poles of three faces of the rhombic dodecahedron.

Join C_1d_5, C_2d_2 , and C_3d_1 meeting in o_1 ; o_1 is the projection of the pole of a face of the octahedron.

(Fig. 43*, Plate IV.*)— $B_1, E_1, F_1, G_1, H_1, K_1, L_1, M_1, N_1, P_1, Q_1$ represent the poles of nearly all the known four-faced cubes lying in the arc of the zone d_5C_3 ; B_2 , &c., in C_3d_2 ; B_3 , &c., in C_1d_2 ; B_4 , &c., in C_2d_5 ; B_5 , &c., in C_2d_5 ; and B_6 , &c., in C_1d_5 . Six poles of each four-faced cube in the octant at equal distances from C_1, C_2 , and C_3 .

Rules for finding the position of B_1, E_1 , &c., will be given hereafter.

$b_1, e_1, f_1, g_1, h_1, k_1$, and l_1 ; b_2, e_2 , &c., l_2 ; and b_3, e_3 , &c., l_3 , three poles of each three-faced octahedron, lying at equal distances from o_1 , in the arcs of zones represented respectively by o_1d_5, o_1d_2 , and o_1d_1 .

$b_1, e_1, f_1, g_1, h_1, k_1, l_1, m_1, n_1, o_1, p_1$, and q_1 ; b_2, e_2 , &c., q_2 ; b_3, e_3 , &c., q_3 , three poles of each twenty-four-faced trapezohedron, lying at equal distances from o_1 , in arcs of zones represented by o_1C_3, o_1C_1 , and o_1C_2 respectively.

Lastly $A_1, B_1, E_1, F_1, G_1, H_1, K_1, L_1, M_1, N_1, P_1, Q_1, R_1, S_1, T_1, U_1$; A_2, B_2 , &c., U_2 ; A_3, B_3 , &c., U_3 ; A_4, B_4 , &c., U_4 ; A_5, B_5 , &c., U_5 ; and A_6, B_6 , &c., U_6 , six poles of the six-faced octahedron; the poles of each particular six-faced octahedron being similarly situated in each of the six triangles $d_5o_1C_3, d_2o_1C_3, d_2o_1C_1, d_5o_1C_2, d_1o_1C_2$, and $d_1o_1C_1$ respectively.

130. To find geometrically the position of any pole on the gnomonic projection (fig. 43*, Plate IV.*).

In (fig. 44*, Plate IV.*).—Let AC_3, AC_2 , and AC_1 be three adjacent cubical axes, rectangular at A .

Let $AC_3=1$. Take AN in AC_1 produced equal to n .

AM in AC_2 produced equal to m .

Join $C_3N, NM, MC_3, C_3C_2, C_2C_1$, and C_1C_3 .

Then C_2MN is the plane $1mn$, and $C_1C_3C_2$ is the plane of the gnomonic projection.

Through A draw AG perpendicular, C_3M meeting C_2C_3 in g , AH perpendicular C_3N , cutting C_1C_3 in h , and AK perpendicular to C_1C_2 , cutting C_1C_2 in k .

h, g , and k are the projections on $C_1C_2C_3$ of H, G , and K . Join NG, MH , and C_3K in the plane NMC_3 , meeting in F ; also join AF . Then, as in § 117, F is the pole of $1mn$, G of $1m\infty$, H of $1\infty n$, and K of $\infty 1 \frac{n}{m}$.

Therefore on the plane of projection, $C_1C_2C_3$, g is the projection of the pole of $1m\infty$, h of $1\infty n$, and k of $\infty 1 \frac{n}{m}$; hC_2 of the line HM , kC_3 of the line KC_3 , gC_1 of the line GN .

f , where hC_2 , kC_3 , and gC_1 meet, will be the pole of $1mn$. Through h , in the plane NAC_3 , draw hE perpendicular to AC_3 . Let angle $hAC_3 = \lambda_2$. Then since angle $AHN = 90^\circ$, angle $ANH = \lambda_2$.

In triangle NAC_3 $\tan ANC_3 = \frac{AC_3}{AN}$ or $\tan \lambda_2 = \frac{1}{n}$

In triangle AhE $\tan hAE = \tan \lambda_2 = \frac{hE}{AE}$

Hence $\frac{hE}{AE} = \frac{1}{n}$ and $hE = \frac{AE}{n} = \frac{AC_3 - 2C_3}{n} = \frac{1 - EC_3}{n}$

But by similar triangles hEC_3 , C_1AC_3 ,

$\frac{hE}{EC_3} = \frac{C_1A}{AC_3} = \frac{1}{1}$. Therefore $hE = EC_3$;

and $EC_3 = \frac{1 - EC_3}{n}$ and $nEC_3 = 1 - EC_3$.

Whence $EC_3 = \frac{1}{n+1}$

But by similar triangles C_1AC_3 , hEC_3 ,

$\frac{AC_3}{EC_3} = \frac{C_1C_3}{hC_3}$ but $AC_3 = 1$ and $EC_3 = \frac{1}{n+1}$

Hence $\frac{C_1C_3}{hC_3} = n+1$, and $C_3h = \frac{C_1C_3}{n+1}$

Hence h , the pole of $1 \infty n$, is found by taking the point h in C_1C_3 , so that $C_3h = \frac{C_1C_3}{n+1}$

Again since $\tan \lambda_2 = \frac{1}{n}$ and angle $hAC_3 = \lambda_2$, if the angular elements be given, C_1C_3 is the chord of 90° and h is the point where the angle λ_2 protracted from A meets C_1C_3 , considering C_3 as zero.

The chord of 90° marked as a protractor is obtainable from any mathematical instrument maker, or may be readily marked on the chord of 90° by using any form of protractor.

Similarly it may be shown that $gC_3 = \frac{C_2C_3}{m+1}$, and that g is the point where the angle λ_3 is marked on C_2C_3 as the chord of 90° , C_3 being zero; and $\tan \lambda_3 = \frac{1}{m}$. Also $kC_2 = \frac{C_1C_2}{\frac{m}{n} + 1}$, k being

the point where the angle λ_1 is marked on the chord of 90° , C_2 being zero, and $\tan \lambda_1 = \frac{m}{n}$.

Join C_1g , C_2h , and C_3k . f , the point where these three lines meet, is the pole of the face of the six-faced octahedron

whose angular elements are p_3 and λ_3 , or whose indices are $l\ m\ n$.

131. To construct a map of all the forms of the octahedral system on a face of an octahedron comprised in an octant of the sphere of projection.

(Fig. 43*, Plate IV.*) Describe any equilateral triangle $C_1C_2C_3$.

Bisect C_1C_2 in d_1 , C_1C_3 in d_2 , and C_2C_3 in d_3 .

Then C_3 is the pole of $1\ \infty\ \infty$, C_2 of $\infty\ 1\ \infty$, and C_1 of $\infty\ \infty\ 1$, three poles of the cube.

d_1 is the pole of $\infty\ 1\ 1$, d_2 of $1\ \infty\ 1$, and d_3 of $1\ 1\ \infty$, three poles of the rhombic dodecahedron.

Join C_1d_5 , C_2d_2 , and C_3d_1 meeting in o . Then o is the pole of the face of the octahedron whose symbol is $1\ 1\ 1$.

To place on this octant six poles of the six-faced octahedron whose indices are $1, \frac{4}{3}, 2$.

In this case $\lambda_3 = 36^\circ\ 52'$, $\lambda_2 = 26^\circ\ 34'$, and $\lambda_1 = 33^\circ\ 41'$.

Graduate each of the lines C_3d_2 , C_3d_5 , C_1d_2 , C_1d_1 , C_2d_1 , and C_2d_5 , from o to 45° ; considering C_1C_2 , C_2C_3 and C_1C_3 as chords of 90° , and making the three points C_1 , C_2 , C_3 each zero, as described in § 132.

Let $C_3F_1 = 36^\circ\ 52' = C_3F_2 = C_1F_3 = C_2F_4 = C_2F_5 = C_1F_6$

$C_3H_1 = 26^\circ\ 34' = C_3H_2 = C_1H_3 = C_2H_4 = C_2H_5 = C_1H_6$

$C_3G_1 = 33^\circ\ 41' = C_3G_2 = C_1G_3 = C_1G_4 = C_2G_5 = C_1G_6$

Then E_1 is the intersection of C_1F_1 , C_2H_2 , C_3G_3

E_2 " of C_1H_1 , C_2F_2 , C_3G_6

E_3 " of C_1G_1 , C_2F_3 , C_3H_3

E_4 " of C_1F_1 , C_2G_2 , C_3H_5

E_5 " of C_1H_1 , C_2G_3 , C_3F_5

E_6 " of C_1G_1 , C_2H_3 , C_3F_6

E_1 , E_2 , E_3 , E_4 , E_5 , and E_6 will be six poles of the six-faced octahedron whose indices are $1, \frac{4}{3}, 2$, and angular elements $\lambda_3 = 36^\circ\ 52'$, $p_3 = 68^\circ\ 12'$. The lines of intersection are not shown in the plate.

(Fig. 43*, Plate IV.*) has marked on it the poles on the octant of a sphere of nearly all the forms of the cubical system which have been observed; all the faces whose poles lie in the same line having their poles on the sphere of projection on the same zone circle.

The angular and linear indices of every form are given in the following table.

Where p_1 , p_2 , and p_3 are the polar distances of each form from the three poles of the poles of the cube, C_1 , C_2 , and C_3 , θ , ϕ , and ψ the supplements of the angles of inclination over the edges of adjacent faces determined as in § 123, 124, 125, and 126.

§ 124, 125, and 126 show how when these angles or any two

of them are determined from observation, the angular or linear elements can be determined from them.

The linear elements have hitherto been almost universally used as a concise means of expressing any form. Their disadvantages will be explained hereafter.

The angular elements are in reality more concise, because they can express the forms they represent to any degree of accuracy which can be derived from observation.

They have also this great advantage, that by the use of angles alone they can express the relations of any form to another without determining the linear elements at all.

Thus in the following table p_1 for any form gives the inclination of the face for which it stands to that of the adjacent face of the cube in any combination of these two forms.

Faces of all the twenty-four faced trapezohedrons lie in the same zone C_1od_5 . Hence the value of p_1 for any of these faces gives the inclination of that face to that of the cube in that zone.

For instance (fig. 43*, Plate IV.*), m_2 is the pole of a face of the twenty-four-faced trapezohedron, for which the value of $p_3=78^\circ 54'$, $\lambda_3=11^\circ 19'$, linear elements 1, 5, 5; l_2 is the pole of another twenty-four-faced trapezohedron, where $p_3=76^\circ 22'$, $\lambda_3=14^\circ 2'$, linear elements 1, 4, 4.

For m_2 ; $p_1=15^\circ 48'$. And for l_2 ; $p_1=19^\circ 28'$.

Hence $54^\circ 44' - 15^\circ 48' = Om_2$; $54^\circ 44' - 19^\circ 28' = Ol_2$; and $19^\circ 28' - 15^\circ 48' = m_2l_2$.

Results procured by simple subtraction when the angular elements are used; but only found by retranslating the linear indices obtained from angular observations of the goniometer back again into angles, by trigonometrical formulæ.

Again, referring to (fig. 43*, Plate IV.*), we see that C_1 , U_3 , Q_3 , H_3 , h_2 , E_2 , f_1 , N_1 , P_1 , H_1 all lie in the same meridional zone.

The values of p_1 for each of these forms enable us to determine the distances of these poles from each other in the zone by simple subtraction of angles.

Table of all the principal forms of the Cubical System.

SIX-FACED OCTAHEDRON.

	$1\ m\ n$	Naumann.	Miller.	λ_3	λ_2	λ_1	p_3	p_2	p_1	θ	ϕ	ψ
<i>A</i>	$1\ \frac{6}{5}\ \frac{6}{5}$	64 0 $\frac{6}{5}\ \frac{6}{5}$	64 63 1	44° 33'	0° 54'	0° 55'	89° 72'	45° 27'	44° 33'	58° 26'	0° 54'	1° 16'
<i>B</i>	$1\ \frac{6}{5}\ \frac{6}{5}$	$\frac{6}{5}\ 0\ \frac{6}{5}$	5 4 3	38 40	30 58	36 52	64 54	55° 33'	45° 0	11 29	11 29	50 12
<i>E</i>	$1\ \frac{4}{3}\ \frac{4}{3}$	$0\ \frac{4}{3}\ 0$	4 3 2	36 52	26 34	33 41	68 12	56° 9	42° 2	15 5	15 5	43 36
<i>F</i>	$1\ \frac{4}{3}\ \frac{4}{3}$	$0\ \frac{4}{3}\ 0$	4 3 1	36 52	14 2	18 26	78 41	53° 58	38 20	32 12	15 57	22 37
<i>G</i>	$1\ \frac{1}{2}\ \frac{1}{2}$	$0\ \frac{1}{2}\ 0$	15 11 7	36 15	25 1	32 28	69 23	56° 24	41 0	16 22	16 22	41 15
<i>H</i>	$1\ \frac{1}{2}\ \frac{1}{2}$	$0\ \frac{1}{2}\ 0$	3 2 1	33 41	18 26	26 34	74 30	57° 41	36 42	21 47	21 47	31 0
<i>K</i>	$1\ \frac{1}{2}\ \frac{1}{2}$	$0\ \frac{1}{2}\ 0$	8 5 1	32 0	7 8	11 19	83 57	58° 12	32 31	34 42	25 50	12 6
<i>L</i>	$1\ \frac{1}{2}\ \frac{1}{2}$	$0\ \frac{1}{2}\ 0$	5 3 1	30 58	11 29	18 26	80 16	59° 32	32 19	27 40	27 40	19 28
<i>M</i>	$1\ \frac{1}{2}\ \frac{1}{2}$	$0\ \frac{1}{2}\ 0$	10 6 1	30 58	5 43	9 28	85 6	59° 10	31 19	35 10	27 58	9 48
<i>N</i>	$1\ 2\ 10$	$0\ 0\ 5$	4 2 1	26 34	14 2	26 34	77 24	64° 7	29 12	17 45	35 57	25 13
<i>P</i>	$1\ \frac{1}{2}\ \frac{1}{2}$	$0\ \frac{1}{2}\ 0$	10 5 1	26 34	5 43	11 19	84 53	63° 33	27 1	29 11	36 43	10 14
<i>Q</i>	$1\ \frac{1}{2}\ \frac{1}{2}$	$0\ \frac{1}{2}\ 0$	11 5 3	24 26	15 15	30 58	76 3	66° 19	27 56	13 3	39 51	27 53
<i>R</i>	$1\ \frac{1}{2}\ \frac{1}{2}$	$0\ \frac{1}{2}\ 0$	16 7 4	23 38	14 2	29 45	77 6	67° 0	26 45	13 36	41 37	25 48
<i>S</i>	$1\ \frac{1}{2}\ \frac{1}{2}$	$0\ \frac{1}{2}\ 0$	7 3 1	23 12	18 26	18 26	82 31	67° 1	24 19	21 15	43 13	14 58
<i>T</i>	$1\ 1\ 1$	$0\ 0\ 0$	21 7 5	18 26	13 24	35 32	77 16	72° 2	22 16	7 8	51 44	25 28
<i>U</i>	$1\ 1\ 1$	$0\ 0\ 0$	8 2 1	14 2	7 8	26 34	83 5	76° 4	15 37	9 46	61 26	13 50

THREE-FACED OCTAHEDRON.

	$1\ m\ n$	Naumann.	Miller.	λ_3	λ_2	λ_1	p_3	p_2	p_1	θ	ϕ	ψ
<i>b</i>	$1\ \frac{6}{5}\ \frac{6}{5}$	$\frac{6}{5}\ 0\ \frac{6}{5}$	65 65 1	45° 0'	44° 33'	44° 33'	55° 9'	54° 32'	54° 32'	0° 43'	0° 0'	69° 42'
<i>e</i>	$1\ \frac{1}{2}\ \frac{1}{2}$	$0\ \frac{1}{2}\ 0$	5 5 4	45° 0	38 40	38 40	60 30	52° 1	52° 1	9 59	0 0	59 0
<i>f</i>	$1\ \frac{1}{2}\ \frac{1}{2}$	$0\ \frac{1}{2}\ 0$	3 3 2	45° 0	33 41	33 41	64 46	50° 14	50° 14	17 20	0 0	50 29
<i>g</i>	$1\ \frac{1}{2}\ \frac{1}{2}$	$0\ \frac{1}{2}\ 0$	7 7 4	45° 0	29 45	29 45	68 0	49° 2	49° 2	22 55	0 0	44 0
<i>h</i>	$1\ \frac{1}{2}\ \frac{1}{2}$	$0\ \frac{1}{2}\ 0$	2 2 1	45° 0	26 34	26 34	70 32	48° 11	48° 11	27 16	0 0	38 57
<i>k</i>	$1\ 1\ 1$	$0\ 0\ 0$	3 3 1	45° 0	18 26	18 26	76 44	46° 30	46° 30	37 52	0 0	26 32
<i>l</i>	$1\ 1\ 1$	$0\ 0\ 0$	4 4 1	45° 0	14 2	14 2	79 59	45° 52	45° 52	43 21	0 0	20 3

OCTAHEDRON.

	$1\ m\ n$	Naumann.	Miller.	λ_3	λ_2	λ_1	p_3	p_2	p_1	θ	ϕ	ψ
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132. *Hemihedral or Half-symmetrical Forms of the Cubic System.*

In the holohedral or perfectly symmetrical forms of the cubical system, the solid form of the crystal is bounded by the lines where any one plane or face is intersected by the adjacent planes or faces. There are, however, symmetrical forms where half the number of the holohedral faces are omitted, the planes of the remaining faces forming a solid by the intersection of the adjacent planes.

These, called *hemihedral* or *half-symmetrical* faced forms, are of two kinds,—the *inclined*, in which no one face is parallel to the other; and the *parallel*, in which the faces are parallel in pairs.

133. The *inclined* hemihedral forms are the *tetrahedron* (figs. 15 and 16, Plate III.), the *twelve-faced trapezohedron* (figs. 17 and 18), the *four-faced tetrahedron* (figs. 19 and 20), and the *six-faced tetrahedron* (figs. 21 and 22); these being the hemihedral forms respectively derived from the *octahedron*, *three-faced octahedron*, *twenty-four-faced trapezohedron*, and *six-faced octahedron*, half of whose faces are produced to meet each other.

There are two hemihedral forms with *parallel* faces,—the *twelve-faced pentagon*, derived from the *four-faced cube* (figs. 23 and 24), and the *irregular twenty-four-faced trapezohedron*, derived from the *six-faced octahedron*.

The *cube* and *rhombic dodecahedron* do not produce *hemihedral* forms, according to the laws of symmetry by which the preceding are formed.

134. The *tetrahedron* (figs. 15 and 16, Plate III.) is formed by taking half the faces of the octahedron (fig. 7, Plate I.), in the following order,— $C_1C_2C_3$, $C_1C_5C_4$, $C_2C_5C_6$, and $C_4C_3C_6$, and producing these planes to intersect in the lines O_4O_2 , O_2O_5 , O_2O_7 , O_4O_5 , O_4O_7 , and O_7O_5 . Referring to (fig. 14, Plate II.), we see that these edges are diagonals of the square faces of the cube in which the octahedron is inscribed, one edge for each face of the cube.

The *tetrahedron* is therefore geometrically inscribed in the same cube in which the octahedron, from which it is derived, is also inscribed. (Fig. 16, Plate III.) shows the face of the octahedron shaded on the corresponding face of the tetrahedron.

Since O_2O_4 , O_2O_5 , and O_4O_5 are diagonals of equal squares, each face of the tetrahedron is an equilateral triangle, $O_2O_4O_5$ (fig. 33, Plate IV.). If we bisect the three sides of this equilateral triangle in the points C_1 , C_2 , and C_3 , and join these points, the equilateral triangle $C_1C_2C_3$ will be a face of the octahedron.

If, therefore, we describe an equilateral triangle (fig. 33, Plate IV.), having each of its sides equal O_4O_5 , (fig. 27, Plate IV.), four such triangles joined together will form the net of a tetrahedron which may be inscribed in the cube, each of whose faces equal the square $O_1O_4O_8O_5$ (fig. 27, Plate IV.).

Besides the tetrahedron just described, another in all respects similar and equal to the former, except as regards its position in the cube, may be formed by producing the four faces of the octahedron $C_1C_2C_5$, $C_1C_3C_4$, $C_2C_3C_6$, and $C_5C_4C_6$ (omitted in the former case), to meet each other. It is customary to call one of these *tetrahedrons* the *positive*, and the other the *negative*. Crystals of the following minerals have faces parallel to those of the tetrahedron :—

Blende (sulphuret of zinc), boracite, diamond, eulytine (bismuth blende), fahlerz (grey copper), pharmacosiderite (arseniate of iron), rhodizite, tennantite, and tritonite.

Naumann's symbol for the *tetrahedron* is $\frac{O}{2}$, Miller's $\kappa 111$.

135. The *twelve-faced trapezohedron* is a half-symmetrical form with inclined faces derived from the *three-faced octahedron*, bounded by twelve equal and similar trapezohedrons (figs. 17 and 18, Plate III.). It is also called the *deltoidal dodecahedron*, the *trapezoidal dodecahedron*, and the *hemi-tri-octahedron*.

It is formed by producing the three faces of the three-faced octahedron corresponding to each face of the octahedron which are produced to form the tetrahedron, to form a solid by their intersection with each other.

Thus, comparing (figs. 17 and 18, Plate III.), with (fig. 6, Plate I.), the three faces meeting respectively in o_1 , o_3 , o_6 , and o_8 of the three-faced octahedron, are produced to meet in the points W_2 , W_4 , W_5 , and W_7 , making, by their intersections, a twelve-faced trapezohedron bounded by twelve equal and similar trapeziums, $W_2C_1o_1C_3$, $W_4C_1o_1C_2$, &c.

If we call this the *positive twelve-faced trapezohedron*, the *negative* will be formed by the twelve faces of the three-faced octahedron which meet in groups of three in the points o_2 , o_4 , o_5 , and o_7 .

To obtain a face of the twelve-faced trapezohedron geometrically from the three-faced octahedron from which it is derived.

Describe the (fig. 29, Plate IV.), as previously shown in § 35, for determining the face of the three-faced octahedron. Produce C_1A to C_6 , and O_1D_5 to O_5 . Take $AC_6 = D_5O_5 = C_1A$. Join C_6O_5 and AO_5 .

Produce Md_5 to meet AO_5 in W_5 . Join C_6W_5 .

Then (fig. 32, Plate IV.) $o_1C_2C_3$ being a face of the three-faced octahedron, bisect C_2C_3 in d_5 . Join o_1d_5 , and produce it to W_5 , making $o_1d_5W_5 = o_1d_5W_5$ (fig. 29, Plate IV.). Join C_2W_5 and C_3W_5 .

Then the trapezium $o_1C_3W_5C_2$ is a face of the *twelve-faced trapezohedron* derived from the *three-faced octahedron* whose face is $o_1C_2C_3$.

Twelve of these trapeziums form a net for the twelve-faced trapezohedron which can be inscribed in the cube whose faces are equal to the square $O_1O_4O_8O_5$ (fig. 27, Plate IV.).

The faces of the three-faced octahedron are shaded on those of the twelve-faced trapezohedron (fig. 18, Plate III.).

The twelve-faced trapezohedron derived from the three-faced octahedron 1 1 2, whose symbols are 2 *O* Naumann, 1 2 2 Miller, and $a^{\frac{1}{2}}$ Brooke; whose symbols are $\frac{1}{2}(1\ 2\ 2)$; $\frac{2O}{2}$ Naumann, $\kappa\ 1\ 2\ 2$ Miller, $\frac{1}{2}(a^{\frac{1}{2}})$ Brooke, occurs parallel to faces of crystals of blende, diamond, and pharmacosiderite.

One derived from the three-faced octahedron 1 1 $\frac{3}{2}$, $\frac{3}{2}O$ Naumann, 2 3 3 Miller, and $a^{\frac{2}{3}}$ Brooke, whose symbols are respectively $\frac{1}{2}(1\ 1\ \frac{3}{2})$; $\frac{\frac{3}{2}O}{2}$; $\kappa\ 2\ 3\ 3$; and $\frac{1}{2}(a^{\frac{2}{3}})$, occurs parallel to faces of crystals of fahlerz.

136. The *three-faced tetrahedron* is a half-symmetrical form, with inclined faces derived from the *twenty-four-faced trapezohedron*. It is bounded by twelve equal and similar isosceles triangles (figs. 19 and 20, Plate III.).

It is also called the *trigonal dodecahedron*, *hemi-icositetrahedron*, *triakis-tetrahedron*, *pyramidal tetrahedron*, and *kuproid*.

It is formed by producing the three faces of the *twenty-four-faced trapezohedron*, corresponding to each face of the octahedron which are produced to form the tetrahedron, to form a solid by their intersection.

Thus, comparing (figs. 19 and 20, Plate III.) with (fig. 4, Plate I.), the three faces of the twenty-four-faced trapezohedron, meeting respectively in o_1 , o_3 , o_6 , and o_8 (fig. 4), are produced to meet in the points O_2 , O_4 , O_5 , and O_7 (figs. 19 and 20, Plate III.), making by their intersections a *three-faced tetrahedron*, bounded by twelve equal and similar isosceles triangles, $O_4O_2o_1$, $O_4O_5o_1$, &c.

If we call this the *positive three-faced octahedron*, the *negative* will be formed by the twelve faces of the twenty-four-faced trapezohedron which meet in groups of three in the points o_2 , o_4 , o_5 , and o_7 .

To obtain a face of the three-faced tetrahedron geometrically from the twenty-four-faced trapezohedron from which it is derived. Describe the (fig. 31, Plate IV.) as previously constructed, § 61, for determining a face of the twenty-four-faced trapezohedron. Produce C_1A to C_6 , O_1D_5 to O_5 ; make $AC_6 = D_5O_5 = AC_1$. Join C_6O_5 , AO_5 . Then it will be found that O_1d_5 produced will cut C_6O_5 in O_5 .

Let $C_1d_1o_1d_2$ (fig. 39) be the face of the twenty-four-faced trapezohedron derived from (fig. 31, Plate IV.).

Produce o_1d_2 to O_2 , and O_1d_1 to O_4 , making $o_1d_2O_2$ and $o_1d_1O_4$ equal to $o_1d_5O_5$ (fig. 31). Join O_4O_2 ; this line will pass through C_1 .

Then $O_4O_2o_1$ is a face of the *three-faced octahedron* derived from that of the *twenty-four-faced trapezohedron* whose face is $C_1d_1o_1d_2$.

Twelve of these isosceles triangles form a net for the *three-faced tetrahedron* which can be inscribed in the cube whose faces are equal to the square $O_1O_4O_8O_5$ (fig. 27, Plate IV.).

The faces of the twenty-four-faced trapezohedron are shaded on those of the three-faced tetrahedron (fig. 20, Plate IV.).

The following curious reciprocal relations may be observed between the perfectly symmetrical and half-symmetrical forms of the three-faced octahedron and the twenty-four-faced trapezohedron.

The hemihedral form of the three-faced octahedron is bounded by trapeziums similar to the faces of the twenty-four-faced trapezohedron.

The hemihedral form of the twenty-four-faced trapezohedron is bounded by isosceles triangles like the faces of the three-faced cube.

The three-faced octahedron is formed by placing a three-faced pyramid of equal isosceles triangles on each of the equilateral triangular faces of the regular octahedron as bases. The three-faced tetrahedron is formed in like manner by placing a three-faced pyramid of equal isosceles triangles on each of the equilateral triangular faces of the regular tetrahedron.

The following *three-faced tetrahedrons*, having faces of crystals parallel to them, have been observed in nature:—

$\frac{1}{2}(1 \frac{3}{2} \frac{3}{2})$; $\frac{\frac{3}{2} O \frac{3}{2}}{2}$ Naumann, $\kappa 2 3 3$ Miller, $a^{\frac{3}{2}}$ Brooke; in tennantite.

$\frac{1}{2}(1 2 2)$; $\frac{2 O 2}{2}$ Naumann, $\kappa 1 1 2$ Miller, a^2 Brooke; in boracite, eulytine, fahlerz, and tennantite.

$\frac{1}{2}(1 3 3)$; $\frac{3 O 3}{2}$; $\kappa 1 1 3$; a^3 ; in blende and fahlerz,

$$\frac{1}{2}(1\ 4\ 4); \frac{4\ 0\ 4}{2}; \kappa\ 1\ 1\ 4; a^4; \text{ in blende.}$$

$$\frac{1}{2}(1\ 5\ 5); \frac{5\ 0\ 5}{2}; \kappa\ 1\ 1\ 5; a^5; \text{ in blende.}$$

137. The *six-faced tetrahedron* is a half-symmetrical form with inclined faces derived from the six-faced octahedron. It is bounded by twenty-four equal and similar scalene triangles (figs. 21 and 22, Plate III.).

It is also called the *hemi-hex-octahedron*, *hexakis-tetrahedron*, and *boracitoid*.

It is formed by producing the six faces of the six-faced octahedron, corresponding to each face of the octahedron which are produced to form the tetrahedron, to form a solid by their intersection. Thus, comparing (figs. 21 and 22, Plate III.) with (fig. 3, Plate I.), the six faces of the six-faced octahedron, meeting respectively in o_1, o_3, o_6 , and o_8 (fig. 3, Plate I.), are produced to meet in the points W_2, W_4, W_5 , and W_7 (figs. 21 and 22, Plate III.), making by their intersections a six-faced tetrahedron, bounded by 24 equal and similar scalene triangles, $o_1C_1W_2, o_1C_3W_2$, &c.

If we call this the *positive* six-faced tetrahedron, the *negative* will be formed by the twenty-four faces of the six-faced octahedron which meet in groups of six in the points o_2, o_4, o_5 , and o_7 (fig. 3, Plate I.). To obtain geometrically a face of the six-faced tetrahedron from the six-faced octahedron from which it is derived, describe the (fig. 35, Plate IV.), as previously constructed, § 68, for determining a face of the six-faced octahedron. Produce C_1A to C_6 , O_1D_5 to O_5 ; make $AC_6 = D_5O_5 = C_1A$. Join C_6O_5 and AO_5 . Produce NO_1d_5 to meet AO_5 in W_5 , and join C_6W_5 .

Then (fig. 36, Plate IV.) let $C_1o_1d_2$ be a face of the six-faced octahedron constructed as in § 69.

Produce o_1d_2 to W_2 and make $o_1d_2W_2 = o_1d_5W_5$, fig. 35.

Join C_1W_2 . Then the scalene triangle $o_1W_2C_1$ is a face of the six-faced tetrahedron derived from the six-faced octahedron whose face is $C_1o_1d_2$. Twenty-four such scalene triangles form a net for the six-faced tetrahedron which can be inscribed in the cube whose faces are equal to the square $O_1O_4O_8O_5$ (fig. 27, Plate IV.). The faces of the six-faced octahedron are shaded on those of the six-faced tetrahedron (fig. 22, Plate III.).

The following six-faced tetrahedrons, having faces of crystals parallel to them, have been observed in nature :

$$\frac{1}{2}(1\ \frac{3}{2}\ 3); \frac{3\ 0\ \frac{3}{2}}{2} \text{ Naumann; } \kappa\ 3\ 2\ 1 \text{ Miller; } \frac{1}{2}(b^1\ b^{\frac{1}{2}}\ b^{\frac{1}{3}})$$

Brooke; in crystals of the diamond.

$\frac{1}{2}(1 \frac{5}{3} 5)$ Naumann; $\frac{5}{2} O \frac{5}{3}$; $\kappa 5 3 1$ Miller; $\frac{1}{2}(b^{-1} b^{\frac{1}{3}} b^{\frac{1}{5}})$

Brooke; in crystals of boracite.

By the construction fig. 35, the ratio $\frac{AW_5}{AO_5}$ may be readily determined by plain trigonometry, just as the ratio $\frac{AO_1}{AO_1}$ was in § 73.

It can also be readily determined by geometry of three dimensions. For (fig. 22, Plate III.) W_2 is a point in each of the three planes $C_1o_1d_2$, $C_3o_1d_2$, $C_1o_3d_3$.

Now the equation to the plane $C_1o_1d_2$ referred to rectangular co-ordinates, AC_1 , AC_2 , AC_3 , is

$$\frac{x}{m} + \frac{y}{n} + \frac{z}{1} = 1 \quad (A)$$

$$\text{To the plane } C_3o_1d_2 \text{ is } \frac{x}{1} + \frac{y}{n} + \frac{z}{m} = 1 \quad (B)$$

$$\text{To the plane } C_1o_3d_3 \text{ is } -\frac{x}{n} - \frac{y}{m} + \frac{z}{1} = 1 \quad (C). \quad (\text{See fig. 31*}, \text{ and fig. 32*}, \text{ Plate IV.}^*)$$

And since x , y , z will be the same for the point W_2 where these planes meet,

$$(A) - (C) \quad x \left(\frac{1}{m} + \frac{1}{n} \right) + y \left(\frac{1}{n} + \frac{1}{m} \right) = 0.$$

$$\text{Therefore } x = -y.$$

$$\text{Also } (A - B) \quad x \left(\frac{1}{m} - 1 \right) + z \left(1 - \frac{1}{m} \right) = 0.$$

$$\text{And } x = z.$$

$$x = -y = z = \frac{1}{1 + \frac{1}{m} - \frac{1}{n}}$$

$$\text{But } AW_2^2 = x^2 + y^2 + z^2 = \frac{3}{\left(1 + \frac{1}{m} - \frac{1}{n} \right)^2}$$

$$\text{And } AW_2 = \frac{\sqrt{3}}{1 + \frac{1}{m} - \frac{1}{n}} = \frac{AO_1}{1 + \frac{1}{m} - \frac{1}{n}}$$

Again, let ω be the angle which the normals of the faces $C_1o_1d_2$, $C_1o_3d_3$ make with each other, or $180^\circ - \omega$ be the angle of inclination of the two faces of the six-faced tetrahedron (fig. 21, Plate III.), over the edge C_1W_2 .

Then since $m n 1$ is the symbol of $C_1o_1d_2$,
and $-n - m 1$ that of $C_1o_3d_3$,

$$\cos \omega = \frac{1 - \frac{2}{mn}}{1 + \frac{1}{m^2} + \frac{1}{n^2}} \quad (\text{See } \S 107.)$$

Or by § 110,

$$\begin{aligned} \cos \omega &= -\cos p_2 \cos p_3 - \cos p_2 \cos p_3 + \cos p_1 \cos p_1 \\ &= \cos^2 p_1 - 2 \cos p_2 \cos p_3. \end{aligned}$$

Which may be computed at once by Byrne's dual logarithms, or thus adapted for ordinary logarithmic computation.

$$\cos \omega = \cos^2 p_1 \left\{ 1 - \frac{2 \cos p_2 \cos p_3}{\cos^2 p_1} \right\}$$

$$\text{Let } \tan a = \frac{2 \cos p_2 \cos p_3}{\cos^2 p_1} = \frac{\cos p_2 \cos p_3}{\cos 60^\circ \cos^2 p_1}$$

$$\text{Then } \cos \omega = \cos^2 p_1 (1 - \tan a) = \frac{\cos^2 p_1 \cos (a + 45^\circ)}{\cos a \sin 45^\circ}$$

138. *Limits of the Form of the Six-faced Tetrahedron.*

As m and n approach in magnitude to unity, the six-faced tetrahedron approximates to the tetrahedron. When $m = n = 1$, the six-faced tetrahedron becomes the tetrahedron, the points W_1, W_2, W_5 , and W_7 (fig. 21, Plate III.) coincide with the points O_1, O_2, O_5 , and O_7 (fig. 15). C_1W_4 and C_1W_2 become the straight line O_2O_4 , &c., and the six faces round each point o_1, o_3, o_6 , and o_8 lie in the same plane.

As m and n increase in magnitude greater than unity, and also in equality to each other, the six-faced octahedron approximates to the cube. When m and n are both infinitely great, it coincides with it. In this case each of the four faces which meet in the six points C_1, C_2, C_3 , &c., C_6 , lie in the same plane. As m approaches to unity, while n increases in magnitude, the six-faced tetrahedron approximates to the rhombic dodecahedron. When $m = 1$ and $n = \infty$ it becomes the rhombic dodecahedron, and the two faces which lie on each side of the twelve lines W_2o_1, W_4o_1, W_5o_1 , &c., lie in the same plane, and the Co and CW become equal.

When m equals unity, while n remains finite, the six-faced tetrahedron becomes the twelve-faced trapezohedron, and the faces on each side of the twelve edges W_2O_1 lie in the same plane, but the edges Co and CW are not equal.

When m and n are equal to each other, both finite and greater than unity, the six-faced tetrahedron becomes the three-faced tetrahedron, and the faces on each side the twelve lines C_1o_1, C_3o_1, C_2o_1 , &c., lie in the same plane. W coincides with O and WCW becomes a straight line. When m remains finite, and n becomes infinite, the six-faced octahedron becomes the four-faced cube, and its scalene triangles become isosceles.

From the above it follows that the cube, rhombic dodeca-

hedron, and four-faced cube, which have no hemihedral forms with inclined faces, are limiting forms of the six-faced tetrahedron.

Also that all the formulæ of the tetrahedron, three-faced tetrahedron, and twelve-faced trapezohedron may be derived from those of the six-faced octahedron by giving the proper values to m and n .

139. Table showing the symbols and formulæ of the half-symmetrical forms which are not included in the table § 131, for the holohedral forms. The letters refer to holohedral forms, § 131.

SIX-FACED OCTAHEDRON.

	Naumann.	Miller.	Brooke.	Ratio $\frac{AW}{AO}$	Angle ω .
$H \frac{1}{2} (1 \frac{3}{2} 3)$	$\frac{3 \ 0 \ \frac{3}{2}}{2}$	$\kappa \ 3 \ 2 \ 1$	$\frac{1}{2} (b^1 b^{\frac{1}{2}} b^{\frac{1}{3}})$	$\frac{3}{4}$	$69^\circ \ 5'$
$L \frac{1}{2} (1 \frac{5}{3} 5)$	$\frac{5 \ 0 \ \frac{5}{3}}{2}$	$\kappa \ 5 \ 3 \ 1$	$\frac{1}{2} (b^1 b^{\frac{1}{3}} b^{\frac{1}{5}})$	$\frac{5}{7}$	$57 \quad 7$

THREE-FACED TETRAHEDRON.

$e \frac{1}{2} (1 \frac{3}{2} \frac{3}{2})$	$\frac{\frac{3}{2} \ 0 \ \frac{1}{2}}{2}$	$\kappa \ 2 \ 2 \ 3$	$\frac{1}{2} (a^{\frac{3}{2}})$	1	$86^\circ \ 38'$
$f \frac{1}{2} (1 \ 2 \ 2)$	$\frac{2 \ 0 \ 2}{2}$	$\kappa \ 1 \ 1 \ 2$	$\frac{1}{2} (a^2)$	1	$70 \quad 32$
$k \frac{1}{2} (1 \ 3 \ 3)$	$\frac{3 \ 0 \ 3}{2}$	$\kappa \ 1 \ 1 \ 3$	$\frac{1}{2} (a^3)$	1	$50 \quad 29$
$l \frac{1}{2} (1 \ 4 \ 4)$	$\frac{4 \ 0 \ 4}{2}$	$\kappa \ 1 \ 1 \ 4$	$\frac{1}{2} (a^4)$	1	$38 \quad 57$
$m \frac{1}{2} (1 \ 5 \ 5)$	$\frac{5 \ 0 \ 5}{2}$	$\kappa \ 1 \ 1 \ 5$	$\frac{1}{2} (a^5)$	1	$31 \quad 35$

TWELVE-FACED TRAPEZOHEDRON.

$f \frac{1}{2} (1 \ 1 \ \frac{3}{2})$	$\frac{\frac{3}{2} \ 0}{2}$	$\kappa \ 2 \ 3 \ 3$	$\frac{1}{2} (a^{\frac{3}{2}})$	$\frac{3}{4}$	$97^\circ \ 51'$
$h \frac{1}{2} (1 \ 1 \ 2)$	$\frac{2 \ 0}{2}$	$\kappa \ 1 \ 1 \ 2$	$\frac{1}{2} (a^{\frac{1}{2}})$	$\frac{2}{3}$	$90 \quad 0$

TETRAHEDRON.

$o \frac{1}{2} (1 \ 1 \ 1)$	$\frac{0}{2}$	$\kappa \ 1 \ 1 \ 1$	$\frac{1}{2} (a^1)$	1	$109^\circ \ 28'$
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140. The *pentagonal dodecahedron* is a half-symmetrical form with parallel faces derived from the four-faced cube. It

is bounded by twelve equal and similar pentagons. These pentagons are, except in one species of the pentagonal dodecahedron, irregular (figs. 23 and 24, Plate III.); four edges or sides of the pentagon being equal, and the fifth unequal. When the five edges are equal, the pentagonal dodecahedron is called the *regular pentagonal dodecahedron*, and is one of the five Platonic bodies.

It is also called the *hemi-hexa-tetrahedron* and *pyritoid*.

It is formed from the *four-faced cube* by taking three out of the six faces (fig. 2, Plate I.) which meet in the points o_1 , o_2 , &c., o_3 ; taking the faces alternately and producing them to form by their intersections a solid by twelve pentagonal faces.

Thus the faces $C_1o_1o_4$, $C_1o_2o_3$, $C_2o_1o_5$, $C_2o_4o_3$, $C_3o_1o_2$, $C_3o_5o_6$, $C_4o_2o_6$, $C_4o_3o_7$, $C_5o_4o_3$, $C_5o_7o_8$, $C_6o_5o_8$, and $C_6o_6o_7$ are produced to form the *positive* pentagonal dodecahedron; the twelve remaining faces to form the *negative* pentagonal dodecahedron. The faces so produced meet in twenty-four equal edges $o_1\delta_1$, $o_1\delta_2$, &c. (figs. 23 and 24, Plate III.); and six other edges, but unequal to the former $\delta_1\delta_9$, $\delta_2\delta_4$, &c.

To obtain a face of the pentagonal dodecahedron geometrically from that of the four-faced cube from which it is derived (fig. 37, Plate IV.), being described as in § 53. Produce C_1d_1 to meet D_1C_2 in δ_1 .

Describe $C_1o_1o_4$ as in § 54, a face of the four-faced cube (fig. 34, Plate IV.). Bisect o_1o_4 in d_1 . Produce C_1d_1 to δ_1 , making $C_1d_1\delta_1 = C_1d_1\delta_1$ (fig. 37). Join $o_1\delta_1$ and $o_4\delta_1$. Through C_1 draw $\delta_1C_1\delta_2$ parallel to o_1o_4 .

Then (fig. 34) take $C_1\delta_2$ and $C_1\delta_4$ each equal $C_2\delta_1$ (fig. 37). Join $o_4\delta_4$ and $o_1\delta_2$.

Then $\delta_4\delta_2o_1\delta_1o_4$ is a face of the pentagonal dodecahedron derived from the four-faced cube whose face is $C_1o_4o_1$.

Twelve such pentagonal faces form a net for the pentagonal dodecahedron which can be inscribed in the cube whose faces are equal to the square $O_1O_4O_8O_5$ (fig. 27, Plate IV.).

The faces of the four-faced cube are shaded on those of the pentagonal dodecahedron (fig. 24, Plate IV.).

The following pentagonal dodecahedrons, having faces of crystals parallel to them, have been observed in nature:—

$\frac{1}{2}[1 \frac{5}{4} \infty]$; $\frac{\infty O \frac{5}{4}}{2}$ Naumann; $\pi \ 5 \ 4 \ 0$ Miller; $\frac{1}{2} b^{\frac{5}{4}}$ Brooke, in pyrite.

$\frac{1}{2}[1 \frac{4}{3} \infty]$; $\frac{\infty O \frac{4}{3}}{2}$; $\pi \ 4 \ 3 \ 0$; $\frac{1}{2} b^{\frac{4}{3}}$, in pyrite.

$\frac{1}{2}[1 \frac{3}{2} \infty]$; $\frac{\infty O \frac{3}{2}}{2}$; $\pi \ 3 \ 2 \ 0$; $\frac{1}{2} b^{\frac{3}{2}}$, in pyrite.

$\frac{1}{2}[1\ 2\ \infty]$; $\frac{\infty\ 0\ 2}{2}$; $\pi\ 2\ 1\ 0$; $\frac{1}{2}b^2$, in cobaltine, cubane, fahlerz, gersdorffite, and pyrite.

$\frac{1}{2}[1\ 3\ \infty]$; $\frac{\infty\ 0\ 3}{2}$; $\pi\ 3\ 1\ 0$; $\frac{1}{2}b^3$, in hauerite, pyrite, and sal ammoniac.

$\frac{1}{2}[1\ 4\ \infty]$; $\frac{\infty\ 0\ 4}{2}$; $\pi\ 4\ 1\ 0$; $\frac{1}{2}b^4$, in cobaltine and fahlerz.

141. *Platonic bodies*.—There are five solid bodies described by the ancient geometers as regular solids. From their mathematical properties having been investigated by Plato and his followers, they are called the *Platonic bodies*. They have all their faces, edges, and angles, whether plane or solid, equal for each body.

They are the *tetrahedron*, bounded by four equal faces, each being an equilateral triangle; the *cube*, bounded by six equal squares; the *octahedron*, bounded by eight equal faces, each being an equilateral triangle; the *pentagonal dodecahedron*, bounded by twelve equal and equilateral pentagons; and the *icosahedron*, by twenty equal faces, each being an equilateral triangle.

The first three, described by Plato himself, have been observed in natural crystals. The last two, described after his death, have not been observed in nature.

The *regular pentagonal dodecahedron* is that particular case of the pentagonal dodecahedron, where the unequal edge, such as $\delta_2\delta_4$ (fig. 23, Plate III.), is equal to the other four δ_2o_1 , $o_1\delta_1$, δ_1o_4 , and $o_4\delta_4$.

In this case $m = \cot \lambda_3 = \frac{1 + \sqrt{5}}{2} = 1.618034$,

but $\cot 31^\circ 43' = 1.618085$.

Hence $\lambda_3 = 31^\circ 43'$ true to minutes.

The value of m is generally determined by continued fractions.

Thus $m = \frac{3\ 4}{2\ 1} = 1.619046$ and $\cot 31^\circ 42' = 1.61914$

$m = \frac{1\ 3}{8} = 1.625$ $\cot 31^\circ 36' = 1.62548$

$m = \frac{8}{5} = 1.6$ $\cot 32^\circ 0' = 1.60033$

The *regular icosahedron* is derived from the particular pentagonal dodecahedron in which the edge $\delta_4\delta_2$ = a line joining the points δ_1 and δ_2 . In this case

$m = \cot \lambda_3 = \frac{3 + \sqrt{5}}{2} = 2.61803 = \cot 20^\circ 54'$,

where the ratio for m expressed in its lowest terms is $m = \frac{3\ 4}{1\ 3}$.

In this particular pentagonal dodecahedron each solid angle

at o_1, o_2 , &c., o_8 , is cut off through the lines $\delta_1\delta_2, \delta_2\delta_5$, and $\delta_5\delta_1$, &c., forming a solid bounded by twenty equilateral triangles,—eight being parallel to the faces of the octahedron inscribed in the dodecahedron, and the remaining twelve faces of the pentagonal dodecahedron.

Ozonam, in his *Mathematical Recreations*, remarks that “The ancient geometricians made a great many geometrical speculations respecting these bodies; and they form almost the whole subject of the last books of Euclid’s *Elements*. They were suggested to the ancients by their believing that these bodies were endowed with mysterious properties, on which the explanation of the most secret phenomena of nature depended.”

142. The *irregular twenty-four-faced trapezohedron* is a half-symmetrical form with *parallel* faces derived from the six-faced octahedron. It is called the irregular twenty-four-faced trapezohedron because its trapezoidal faces have only two equal edges, and to distinguish it from the twenty-four-faced trapezohedron, which is a holohedral form and has the four edges of its trapezoidal faces equal in pairs.

It is bounded by twenty-four irregular trapeziums (figs. 25 and 26, Plate II.).

It is also called the *hemi-octakis-hexahedron*, the *trapezoidal icosi-tetrahedron*, the *dyakis dodecahedron*, the *diploid*, and the *diplopyritoid*.

It is formed from the six-faced octahedron by taking three out of the six faces which meet in o_1, o_2 , &c., o_8 (fig. 31, Plate I.), and producing them to meet each other and form a solid bounded by twenty-four irregular trapeziums.

Thus (fig. 3, Plate I.) the twenty-four faces $C_1o_1d_1, C_2o_1d_5, C_3o_1d_2, C_2o_4d_3, C_1o_4d_1, C_3o_4d_4$, &c., are produced to meet in the points δ_1, δ_2 , &c., δ_{12} (fig. 25, Plate III.), to form the *positive* irregular twenty-four-faced trapezohedron.

The remaining twenty-four-faces if produced will form the *negative* trapezohedron.

To obtain a face of the irregular twenty-four-faced trapezohedron geometrically from that of the six-faced octahedron from which it is derived.—Describe (fig. 35, Plate IV.), as previously constructed for finding a face of the six-faced octahedron, § 68 and § 137. Join C_2N cutting C_1d_1 produced in δ_1 . Let $C_2o_1d_5$ (fig. 38, Plate IV.) be a face of the six-faced octahedron. Produce C_2d_5 to δ_5 , and make $C_2\delta_5\delta_5$, fig. 38, $= C_1d_1\delta_1$ (fig. 35). Join $o_1\delta_5$, on base C_2o_1 , describe the triangle $C_2\delta_1o_1$, having $C_2\delta_1 = C_2\delta_1$ fig. 35, and $o_1\delta_1 = o_1\delta_5$ fig. 38.

$O_1\delta_5C_2\delta_1$ will be a face of the irregular twenty-four-faced trapezohedron, and twenty-four such faces will form a net for the same, which can be inscribed in a cube whose faces are equal to the square $O_1O_5O_8O_4$ (fig. 27, Plate IV.).

The faces of the six-faced octahedron are shaded on those of the irregular twenty-four-faced trapezohedron in (fig. 26, Plate III.).

The following *irregular twenty-four-faced trapezohedrons*, having faces of crystals parallel to them, have been observed in nature.

$$\frac{1}{2}[1 \frac{5}{4} \frac{5}{3}]; \frac{\frac{5}{3} O \frac{5}{4}}{2} \text{ Naumann; } \pi \ 5 \ 4 \ 3 \text{ Miller; } b^{\frac{1}{5}} b^{\frac{1}{4}} b^{\frac{1}{3}}$$

Brooke, in crystals of pyrite.

$$\frac{1}{2}[1 \frac{4}{3} 2]; \frac{2 O \frac{4}{3}}{2}; \pi \ 4 \ 3 \ 2; b^{\frac{1}{4}} b^{\frac{1}{3}} b^{\frac{1}{2}}, \text{ in linnéite.}$$

$$\frac{1}{2}[1 \frac{15}{11} \frac{15}{7}]; \frac{\frac{15}{7} O \frac{15}{11}}{2}; \pi \ 15 \ 11 \ 7; b^{\frac{1}{15}} b^{\frac{1}{11}} b^{\frac{1}{7}}, \text{ in linnéite.}$$

$$\frac{1}{2}[1 \frac{3}{2} 3]; \frac{3 O \frac{3}{2}}{2}; \pi \ 3 \ 2 \ 1; b^{\frac{1}{3}} b^{\frac{1}{2}} b^1, \text{ in cobaltine, hauerite, and pyrite.}$$

$$\frac{1}{2}[1 \frac{3}{2} 5]; \frac{5 O \frac{5}{3}}{2}; \pi \ 5 \ 3 \ 1; b^{\frac{1}{5}} b^{\frac{1}{3}} b^1, \text{ in pyrite.}$$

$$\frac{1}{2}[1 \frac{5}{3} 10]; \frac{10 O \frac{5}{3}}{2}; \pi \ 10 \ 6 \ 1; b^{\frac{1}{10}} b^{\frac{1}{6}} b^1, \text{ in pyrite.}$$

$$\frac{1}{2}[1 \ 2 \ 4]; \frac{4 O \ 2}{2}; \pi \ 4 \ 2 \ 1; b^{\frac{1}{4}} b^{\frac{1}{2}} b^1, \text{ in pyrite.}$$

$$\frac{1}{2}[1 \ 5 \ 10]; \frac{10 O \ 5}{2}; \pi \ 10 \ 5 \ 1; b^{\frac{1}{10}} b^{\frac{1}{5}} b^1, \text{ in pyrite.}$$

143. Let μ be the supplement of the angle of adjacent faces over the edges, such as $C_1\delta_2$, $C_2\delta_1$, $C_3\delta_5$, &c.

ν that over the edges $o_1\delta_1$, $o_1\delta_5$, $o_1\delta_2$, &c.

Then μ is the inclination of normal of face $C_2o_1d_5$ to that of $C_2o_4d_3$, fig. 26, Plate III., but indices of $C_2o_1d_5$ are $m \ 1 \ n$, and of $C_2o_4d_3$ $\bar{m} \ 1 \ n$ (fig. 31*, Plate IV.*).

$$\text{Hence } \cos \mu = \frac{-\frac{1}{m^2} + \frac{1}{1} + \frac{1}{n^2}}{\frac{1}{m^2} + \frac{1}{n^2} + 1}$$

Also ν is the inclination of normal of face $C_2d_5o_1$ to that of $C_1d_1o_1$ (fig. 26 Plate III.), but indices of $C_2d_5o_1$ are $m \ 1 \ n$, and of $C_1d_1o_1$, $n \ m \ 1$ (fig. 31*, Plate IV.*).

$$\text{Hence } \cos \nu = \frac{\frac{1}{mn} + \frac{1}{m} + \frac{1}{n}}{\frac{1}{m^2} + \frac{1}{n^2} + 1}$$

Or, expressing μ and ν in terms of the polar distances $C_2o_1d_5 = p_2p_1p_3$ and $C_2o_4d_5 = -p_2p_1p_3$.

And $\cos \mu = \cos^2 p_1 - \cos^2 p_2 + \cos^2 p_3$,

$C_2d_5o_1 = p_2p_1p_3$ $C_1d_1o_1 = p_3p_2p_1$,

$\cos \nu = \cos p_2 \cos p_3 + \cos p_1p_2 + \cos p_1p_3$;

formulae calculable at once by Byrne's dual logarithms, or easily adapted to logarithmic computation by subsidiary angles.

All the formulae for the *pentagonal dodecahedrons* are immediately derivable from those of the *irregular twenty-four-faced trapezohedron*.

144. *Limits of the Form of the Irregular Twenty-four-faced Trapezohedron.*

As m and n approach in magnitude to unity, the irregular twenty-four-faced trapezohedron approximates to the octahedron; and when m and n both equal unity, it becomes the octahedron. In this case the three planes meeting in the points o_1, o_2 , &c., o_8 (fig. 25, Plate III.), lie in the same plane, and the edges, such as C_1d_1, C_2d_1 , lie in the same line.

As m and n both increase in magnitude and become infinitely great, this form approximates to and becomes the cube. In this case the four planes meeting in C_1, C_2 , &c., C_6 , become the same plane, and the edges, such as $o_4d_1o_1, o_1d_5o_5$, &c., the same straight line.

As m approaches to unity while n increases in magnitude and becomes infinitely great, the form approaches the rhombic dodecahedron. When m equals unity, while n remains finite, the form becomes the three-faced octahedron. When m and n equal each other and are both finite and greater than unity, the form becomes that of the regular twenty-four-faced trapezohedron. Finally, when m remains finite and greater than unity and n becomes infinite, the form becomes that of the pentagonal dodecahedron.

145. As yet the half-symmetrical forms with parallel faces, the pentagonal dodecahedron and the irregular twenty-four-faced trapezohedron have only been found in combination with those of the full symmetrical forms of the cubical system, and never with those of the half-symmetrical forms with inclined faces.

146. For the pentagonal dodecahedrons the following are the values of the angles μ and ν .

E	$\frac{1}{2}$	$[1 \frac{5}{4} \infty]$	$\mu = 77^\circ 19'$	$\nu = 60^\circ 48'$.
F	$\frac{1}{2}$	$[1 \frac{4}{3} \infty]$	$\mu = 73^\circ 44'$	$\nu = 61^\circ 19'$.
G	$\frac{1}{2}$	$[1 \frac{3}{2} \infty]$	$\mu = 67^\circ 23'$	$\nu = 62^\circ 31'$.
H	$\frac{1}{2}$	$[1 2 \infty]$	$\mu = 53^\circ 8'$	$\nu = 66^\circ 25'$.
M	$\frac{1}{2}$	$[1 3 \infty]$	$\mu = 36^\circ 52'$	$\nu = 72^\circ 33'$.
N	$\frac{1}{2}$	$[1 4 \infty]$	$\mu = 28^\circ 4'$	$\nu = 76^\circ 23'$.

For the irregular twenty-four-faced trapezohedrons the following are the values of μ and ν .

$B \frac{1}{2}$	$\left[1 \frac{5}{4} \frac{5}{3}\right]$	$\mu = 68^\circ 54'$	$\nu = 19^\circ 57'$.
$E \frac{1}{2}$	$\left[1 \frac{5}{4} 2\right]$	$\mu = 67^\circ 16'$	$\nu = 26^\circ 17'$.
$G \frac{1}{2}$	$\left[1 \frac{1}{1} \frac{1}{7}\right]$	$\mu = 67^\circ 13'$	$\nu = 28^\circ 32'$.
$H \frac{1}{2}$	$\left[1 \frac{1}{3} 2\right]$	$\mu = 64^\circ 37'$	$\nu = 38^\circ 13'$.
$K \frac{1}{2}$	$\left[1 \frac{1}{2} 8\right]$	$\mu = 63^\circ 37'$	$\nu = 53^\circ 55'$.
$L \frac{1}{2}$	$\left[1 \frac{1}{2} 5\right]$	$\mu = 60^\circ 56'$	$\nu = 48^\circ 55'$.
$M \frac{1}{2}$	$\left[1 \frac{1}{2} 10\right]$	$\mu = 61^\circ 41'$	$\nu = 56^\circ 18'$.
$N \frac{1}{2}$	$\left[1 2 4\right]$	$\mu = 51^\circ 45'$	$\nu = 48^\circ 11'$.
$P \frac{1}{2}$	$\left[1 5 10\right]$	$\mu = 22^\circ 46'$	$\nu = 72^\circ 17'$.

147. Some crystals have a tendency to split in directions parallel to a certain form. This is called a cleavage-plane. If they split readily, the cleavage is called a perfect one. Substances which crystallize in the cubical system have only been observed to split or cleave parallel to the planes of the cube, octahedron, and rhombic dodecahedron.

Minerals whose crystals cleave parallel to the faces of the cube, those printed in italics indicating that the cleavage is easy and perfect :—

<i>Alabandine.</i>	<i>Galena.</i>	Pyrite.
Altaite.	<i>Gersdorffite.</i>	Pyrochlore.
Analcine.	<i>Hauerite.</i>	<i>Salt.</i>
Argentite.	Iridium.	<i>Skutterudite.</i>
Chromite.	Iron.	Smaltine.
Clausthalite.	Lerbachite.	Spinnelle.
<i>Cobaltine.</i>	Linnéite.	Stannine.
<i>Cubane.</i>	<i>Magnetite.</i>	Steinmannite.
Embolite.	<i>Naumannite.</i>	<i>Sylvine.</i>
Franklinite.	<i>Periclas.</i>	<i>Ullmanite.</i>
<i>Gahnite.</i>	Perowskite.	

Minerals whose crystals cleave parallel to the faces of the octahedron :—

Alum.	<i>Diamond.</i>	Grünauite.
Arsenite.	Eisennickelkies.	Magnetite.
Boracite.	Fahlerz.	Sal ammoniac.
Bornite.	<i>Fluor.</i>	Senarmontite.
Chromite.	Franklinite.	Smaltine.
Cuprite.	Gahnite.	Spinnelle.

Minerals whose crystals cleave parallel to the faces of the rhombic dodecahedron :—

Alabandine.	Garnet.	Smaltine.
Amalgam.	<i>Hawyne.</i>	Sodalite.
Argentite.	<i>Ittnerite.</i>	Stannine.
<i>Blende.</i>	Leucite.	Tennantite.
Eulytine.	Skutterudite.	

148. In the following table all substances which crystallize on the cubical system are arranged according to their chemical formulæ; the letters c, o, and d, representing that faces parallel to the cube, octahedron, and rhombic dodecahedron, occur on their crystals. The crystals having faces parallel to other forms have been previously enumerated under those forms. The table is principally taken from Rammelsberg's Crystallographic Chemistry.

Chemical Formulæ of Substances crystallizing on the Cubical System.

Ag, Silver (o c d)	Ni As, Rammelsbergite (o c d)
Au, Gold (o c d)	Co As, Smaltine (o c d)
Cu, Copper (o c)	Co ² As ³ , Skutterudite (o c d)
Fe, Iron (o c)	(Ni Co) ^m As ⁿ
Hg, Mercury (o)	(Co Fe) As, Safflorite (o c)
Ir, Iridium (o c)	Ni'' + Ni(SbAs) Cobaltine (o c)
Pb, Lead (o)	K Fl
Pt, Platinum (c)	Na Fl (c d)
P, Phosphorus (o d)	Ca Fl (o c d) Fluor
C, Diamond (o c d)	K Cl, Sylvine (c o)
Mg, Periclase (o c)	Am Cl, Salammoniac (o c d)
Ni (o c)	Na Cl, Salt (c o d)
Cd (o c d)	Li Cl (c)
Cu , Cuprite (o c d)	Ag Cl, Kerate (c o d)
Sb, Senarmontite (o)	U Cl (c)
As, Arsenite (o)	Cu Cl
U \bar{U} , Pechuran (o)	Co Cl + 8 aq (o c)
Ir + Os, Irite (o)	K Br (c)
Ca + Ti, Perowskite (o d)	Na Br (c)
Ca + 4 B, Rhodozite (o d)	Ag Br, Bromite (c o)
Fe + (Fe Ti), Iserine (o c d)	K I (c o d)
Cu' and Cu' Fe' (o)	Am I (c o d)
Mn', Alabandine (o c d)	Na I (c)
Zn', Blende (o c d)	Zn I (o)
Pb', Galena (o c d)	Pb I (o)
Pb' Fe'	K Cy
Pb' Sb''', Steinmannite (o c)	Am Cy (o c)
Ag', Argentite (o c d)	Na Cy
Mn'', Hauerite (o c d)	Ti Cy + 3Ti ³ N (c)
Fe'', Pyrite (o c d)	Ag Hg, Amalgam (o c d)
Ni', Grünauite (o c)	Ag ⁶ Hg, Arquerite (o)
	Ag Se, Naumannite (c)
	Ag Te, Petzite (c)
	Pb Se, Clausthalite (c)
	Pb Se and Hg Se, Lerbachite (c)

Pb Te , Altaite (c)
 $\text{Mg} + \text{Al}$, Spinelle (o d)
 $\text{Zn} + \text{Al}$, Gahnite (o c)
 $\text{Fe} + \text{Fe}$, Magnetite (o c d)
 $\text{Fe} + \text{Cr}$, Chromite (o)
 $\text{Fe} + \text{Mn}$, Franklinite o c d
 $\text{Al} \text{S}^3 + 18 \text{ aq}$
 $\text{Cr} \text{S}^3 + 15 \text{ aq}$
 $\text{Ba} \ddot{\text{N}}$ (o c)
 $\text{Sr} \ddot{\text{N}}$ (o c)
 $\text{Pb} \ddot{\text{N}}$ (o c)
 $\text{Na} \ddot{\text{Cl}}$ (c o d)
 $\text{Ni} \ddot{\text{Cl}} + 6 \text{ aq}$
 $\text{Co} \ddot{\text{Cl}} + 6 \text{ aq}$
 $\text{Cu} \ddot{\text{Cl}} + 6 \text{ aq}$ (o)
 $\text{K} \ddot{\text{Br}}$ (c o d)
 $\text{Na} \ddot{\text{Br}}$ (c o d)
 $\text{Mg} \ddot{\text{Br}} + 6 \text{ aq}$
 $\text{Zn} \ddot{\text{Br}} + 6 \text{ aq}$
 $\text{Ni} \ddot{\text{Br}} + 6 \text{ aq}$
 $\text{Co} \ddot{\text{Br}} + 6 \text{ aq}$ (o c)
 $\text{Am} \ddot{\text{I}}$ (c)
 $\text{Mg}^3 \ddot{\text{Br}}$
 $\text{Mg}^3 \ddot{\text{B}}^4$, Boracite (c o d)
 $\text{Na} \ddot{\text{B}}^2 + 5 \text{ aq}$, Borax
 $\text{Na} \ddot{\text{H}} + 12 (\text{Na} \ddot{\text{Sb}}) + 7 \text{ aq}$ (o)
 $3(\text{Fe} \ddot{\text{As}} + 4 \text{ aq}) + \text{H}_3 \ddot{\text{Fe}}$, Pharmacosiderite (o c d)
 $\text{Cu}' \text{Fe}''' + 2 \text{ Fe}$, Cubane (c)
 $\text{Cu}^3 \text{Fe}'''$, Bornite (c o d)
 $\text{Co}' \text{Ce}'''$, Linnéite (c o)
 $\text{Pb}^2 \text{As}'''$, Dufrenoy'site (d)
 $\text{R}^4 (\text{Sb}''' \text{As}''')$, Fahlerz (o c d)
 $\text{R} = \text{Pb}, \text{Fe}, \text{Zn}, \text{and } \text{Cu}^4$

$(\text{Ni Co})^3 \text{S}^4$
 $\text{Ni Sb} + \text{Ni}'''$, Ullmanite (o c d)
 $4(\text{Fe}' 2\text{Ca}') + \text{As}'''$, Tennantite (o c d)
 $\text{Na}'^3 \text{Sb}''' + 18 \text{ aq}$ (o d)
 $\text{Fe}' \text{Ni}'$, Eisennickelkies (o)
 $(2 \text{Cu}' + \text{Sn}'') + (\text{Fe}' + \text{Sn}'')$, Stannine (c d)
 $\text{Ni}' + \text{Ni} \text{As}^2$, Gersdorffite (o c)
 $\text{Am Cl} + \text{Mn Cl} + \text{aq}$ (c d)
 $\text{Ca Cl} + 5 \text{Hg Cl} + 8 \text{ aq}$ (o)
 $[2 (\text{K Am}) \text{Cl} + \text{Fe Cl}^3] + 2 \text{ aq}$
 $(\text{Ni Cl} + 2 \text{N H}^3) + \text{aq}$ (o c d)
 $\text{Am Cl} + \text{Sn Cl}^2$ (o c d)
 $\text{K Cl} + \text{Pt Cl}^2$ (o)
 $(\text{Pb Cl} + \text{Pb}) + (\text{Cu Cl} + \text{Cu}) + \text{aq}$, Percylite (o c d)
 $2 \text{Ag Br} + 3 \text{Ag Cl}$, Embolite o c
 $\text{Zn Br} + \text{N H}^3$ (o)
 $\text{Ca Br} + \text{N H}^3$ (o)
 $\text{Ni I} + 3 \text{N H}^3$ (o)
 $\text{K Cy} + \text{Zn Cy}$ (o)
 $\text{K Cy} + \text{Cd Cy}$ (o)
 $\text{K Cy} + \text{Hg Cy}$ (o)
 $\text{K Cy} + \text{Ag Cy}$ (o)
 $\text{K} \ddot{\text{S}} + \text{Al} \text{S}^3 + 24 \text{ aq}$, Alum (o c d)
 $\text{Am} \ddot{\text{S}} + \text{Al} \text{S}^3 + 24 \text{ aq}$
 $\text{K} \ddot{\text{S}} + \text{Fe} \text{S}^2 + 24 \text{ aq}$
 $\text{Am} \ddot{\text{S}} + \text{Fe} \text{S}^3 + 24 \text{ aq}$
 $\text{K} \ddot{\text{S}} + \text{Mn} \text{S}^3 + 24 \text{ aq}$
 $\text{Am} \ddot{\text{S}} + \text{Mn} \text{S}^3 + 24 \text{ aq}$
 $\text{K} \ddot{\text{S}} + \text{Cr} \text{S}^3 + 24 \text{ aq}$
 $\text{Am} \ddot{\text{S}} + \text{Cr} \text{S}^3 + 24 \text{ aq}$
 $3(\text{Fe K}) \ddot{\text{S}} + 2 \text{Fe} \text{S}^3 + 12 \text{ aq}$ (o)
 $\ddot{\text{Bi}} + \ddot{\text{Si}}^3$, Eulytine (o c d)
 $\text{Na} \ddot{\text{Si}} + \text{Al} \ddot{\text{Si}}^3$, Analcine (c)
 $\text{K} \ddot{\text{Si}} + \text{Al} \ddot{\text{Si}}^3$, Leucite (d)
 $\text{R}^3 \ddot{\text{Si}}^2 + \text{R}' \ddot{\text{Si}}$, Garnet (c d)

Where $\dot{R} = \dot{Ca}, \dot{Fe}, \dot{Mn}$,

and $\dot{R}' = \dot{Fe}, \dot{Al}$

$\dot{Ca}^3 \dot{Si}^2 + \dot{Cr} \dot{Si}$, Uwarrowite
(o d)

$(\dot{Mn}, \dot{Fe})^3 \dot{Si}^2 + \dot{Be} \dot{Si} + \dot{Am} S$,
Mn O, Helvin (o)

$\dot{Na} Cl + 3 \dot{Na} \dot{Si} + 3 \dot{al} \dot{Si}$,
Sodaltite, (c d)

$(\dot{Ni} \dot{N} + 2N H^3) + aq$ (o c)

$(\dot{Na} \dot{Ca} + 8Zn \dot{C}) + 8aq$ (o)

$(3\dot{Na} \dot{C} + \dot{Cr} \dot{C}^3) + 9aq$

$\dot{Fe}^3 \dot{As} + \dot{Fe}^3 \dot{As}^2 + 18aq$ (c)

$\dot{Na} \dot{W} + \dot{W} \dot{W}$ (c)

$\dot{Na} \dot{Ac} + 2 \dot{U} \dot{Ac}$ (o)

$C^{12} (H^6 Cl) N$

$C^{12} (H^6 Br) N$ (o)

$C^{20} H^{16} O^2$, Camphor (o)

Substances whose formulæ are
undetermined:—

Hauyne, or Lapis Lazuli, a
silicate of Alumina, Soda,
and Lime (o c d)

Pyrochlore, Titanium ore (o c d)

Tritonite, Silicate of oxides of
Cerium and Lanthanium (c)

Voltaite, Hydrous sulphate of
iron, &c. (o c d)

* * A discussion* followed, in which C. BROOKE, Esq., F.R.S., Professor MORRIS, the HONORARY SECRETARY, and the CHAIRMAN took part ; after which—

The Meeting was adjourned.

* This discussion having been of a very general character, it has not been found necessary to insert it.



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20. 5. 82.

Fig 1. $1.\infty.\infty$.

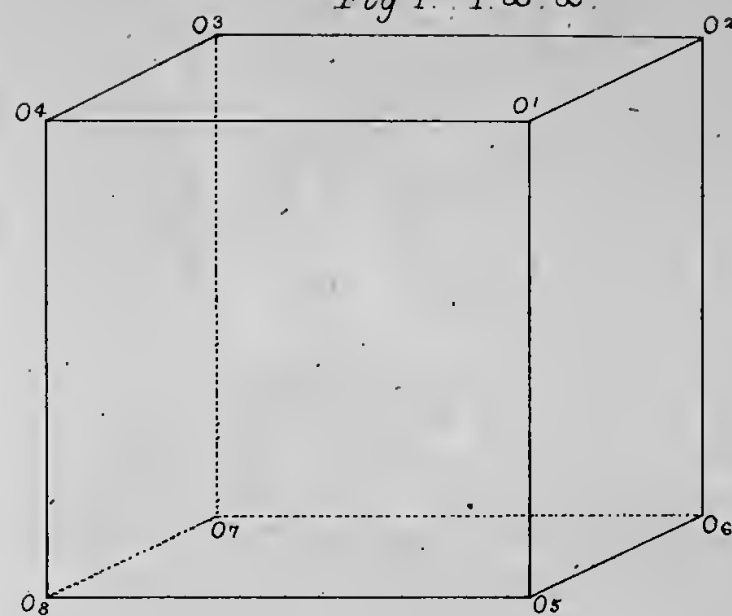


Fig. 2. $1.m.\infty$.

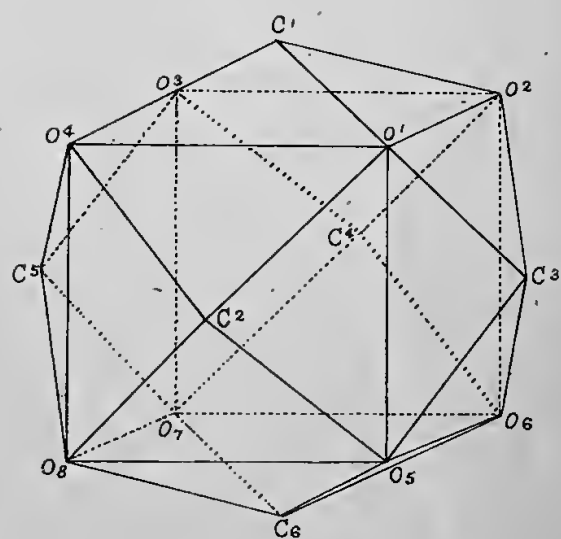


Fig. 3. $1.m.n$.

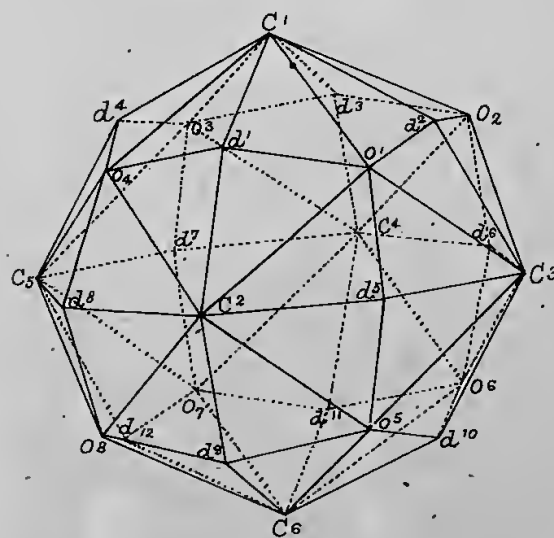


Fig. 4. $1.m.m$.

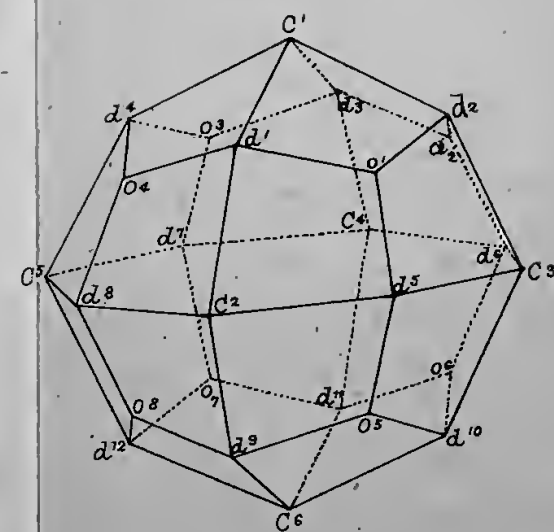


Fig. 5. $1,1.\infty$.

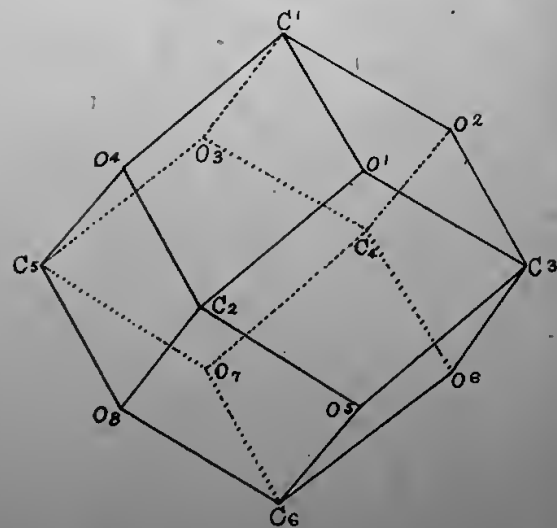


Fig. 6. $1,1.m$.

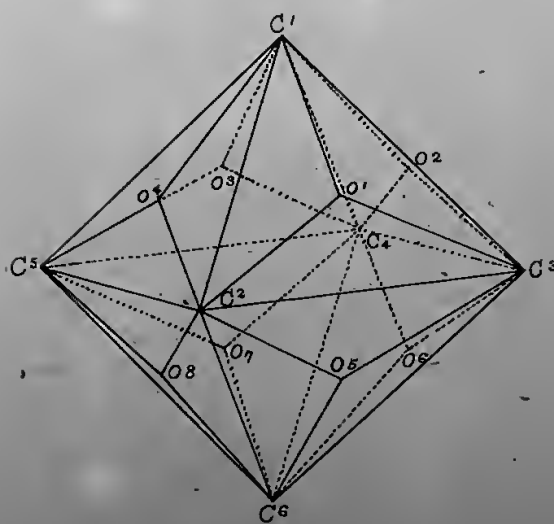
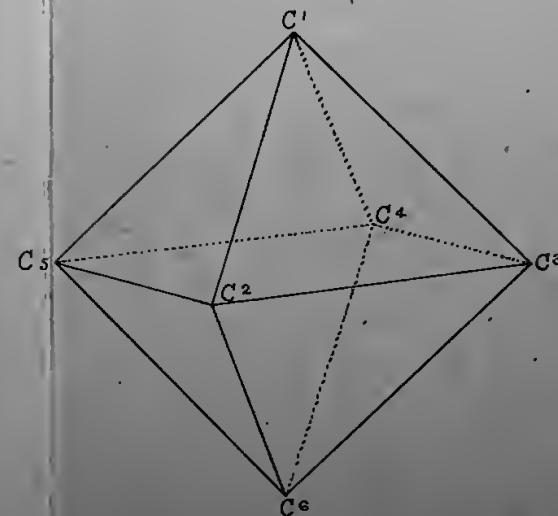


Fig. 7. $1,1,1$.



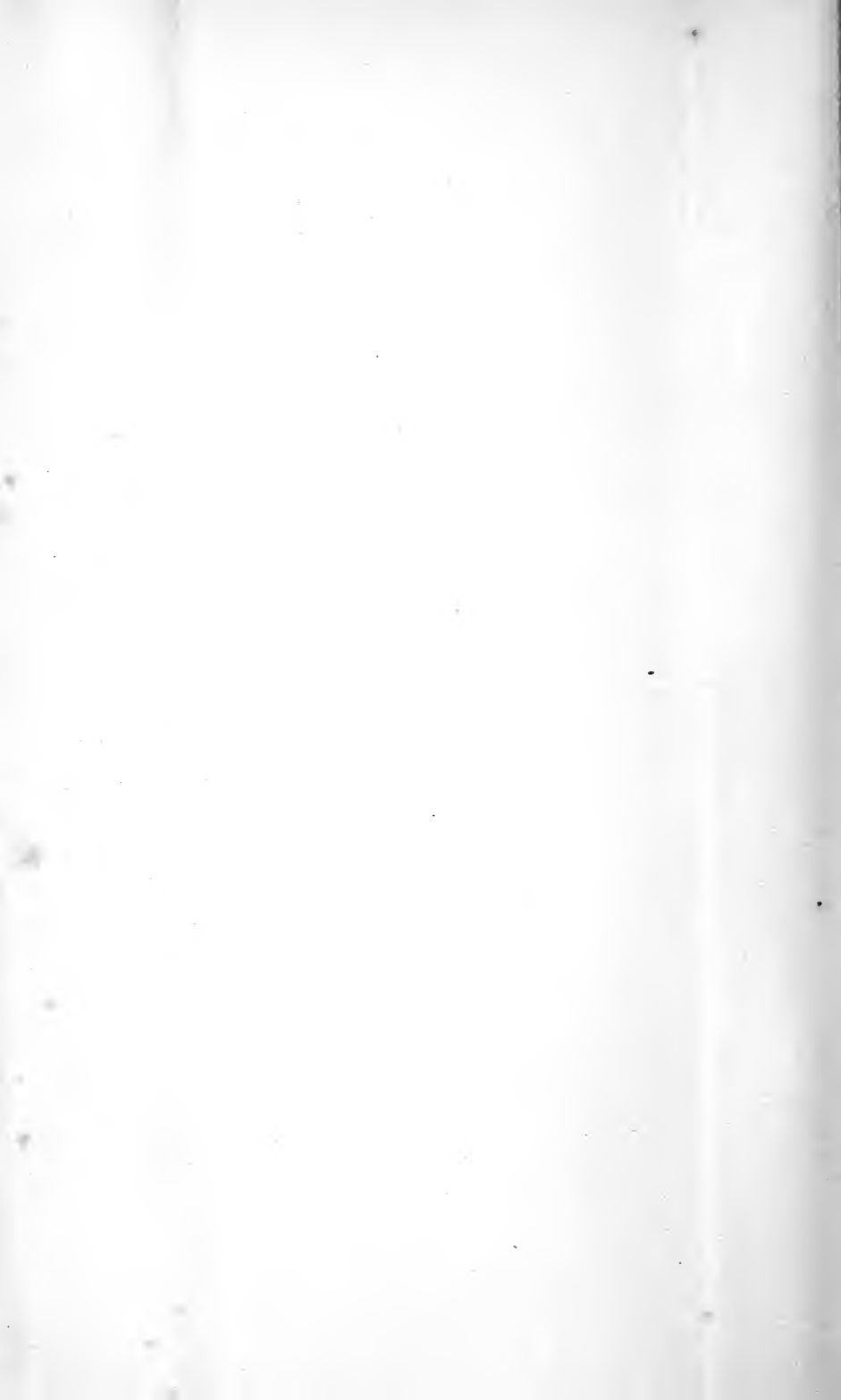


Fig. 8.

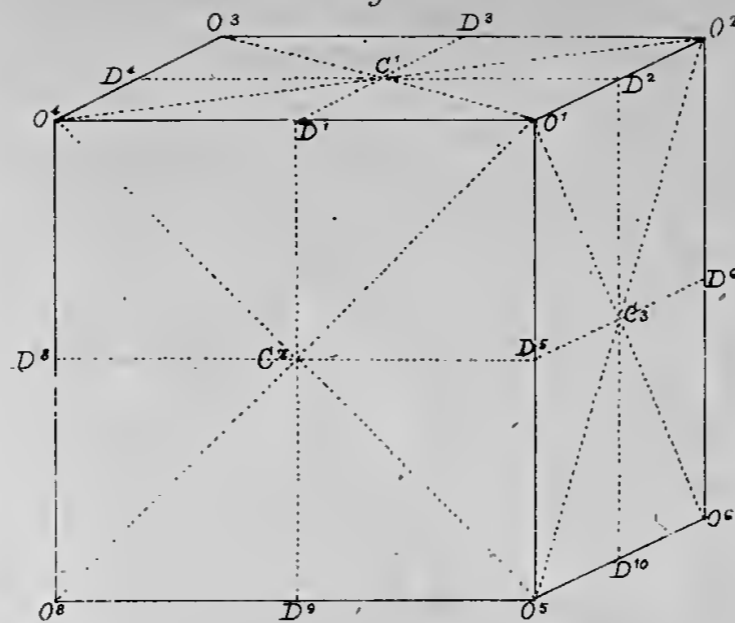


Fig. 9.

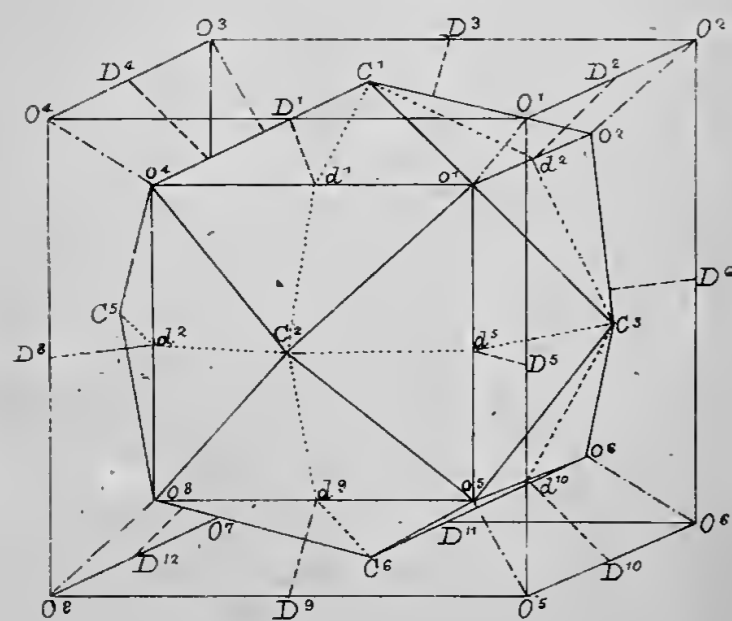


Fig. 10.

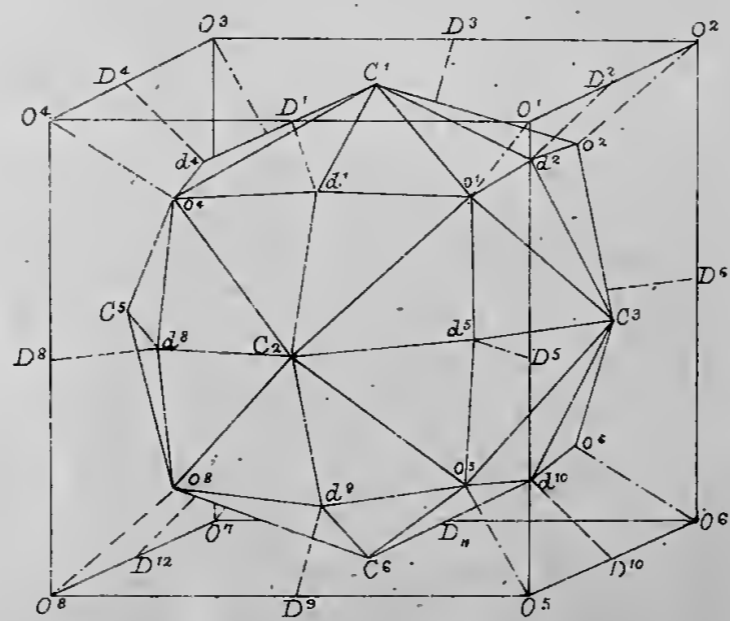


Fig. 11.

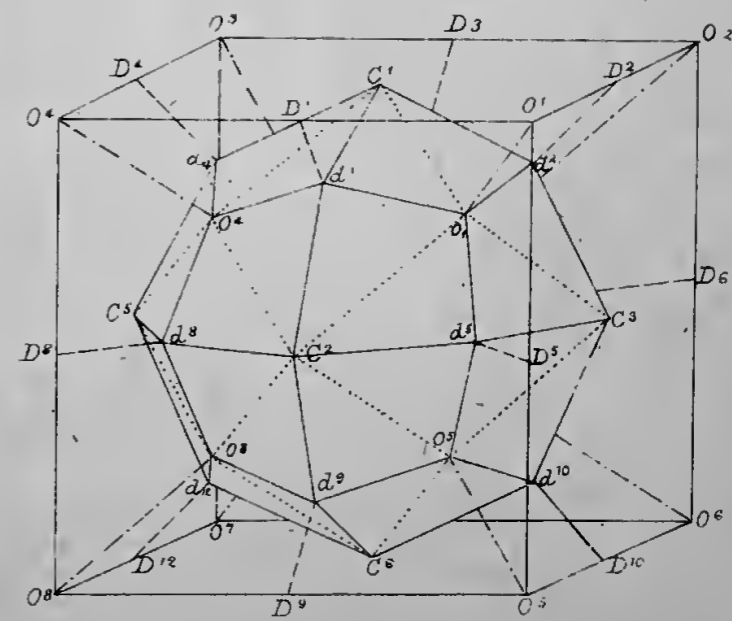


Fig. 12.

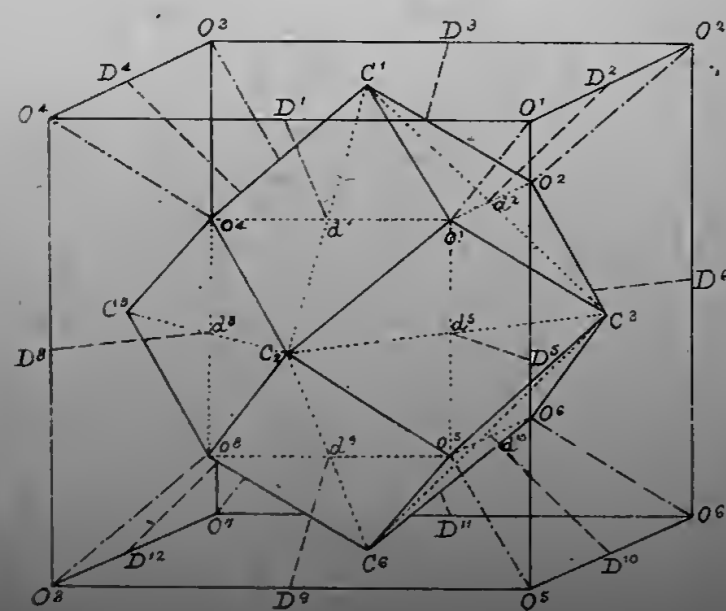


Fig. 13.

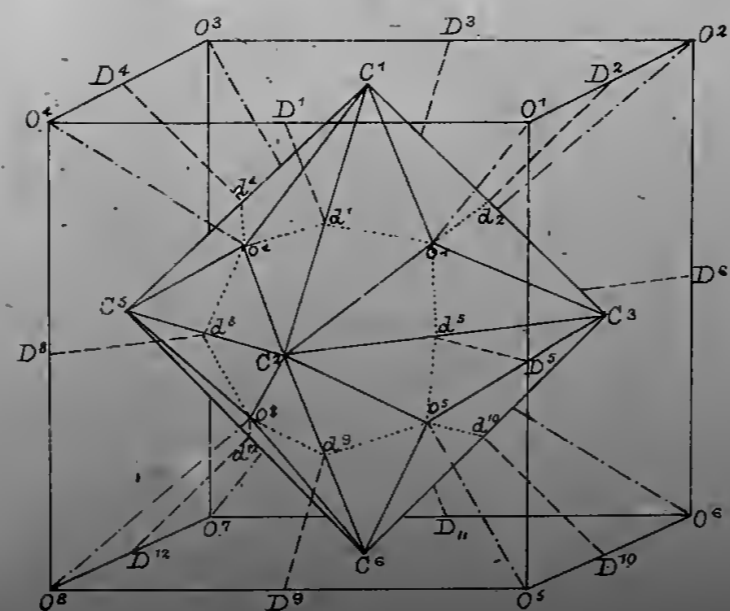


Fig. 14.

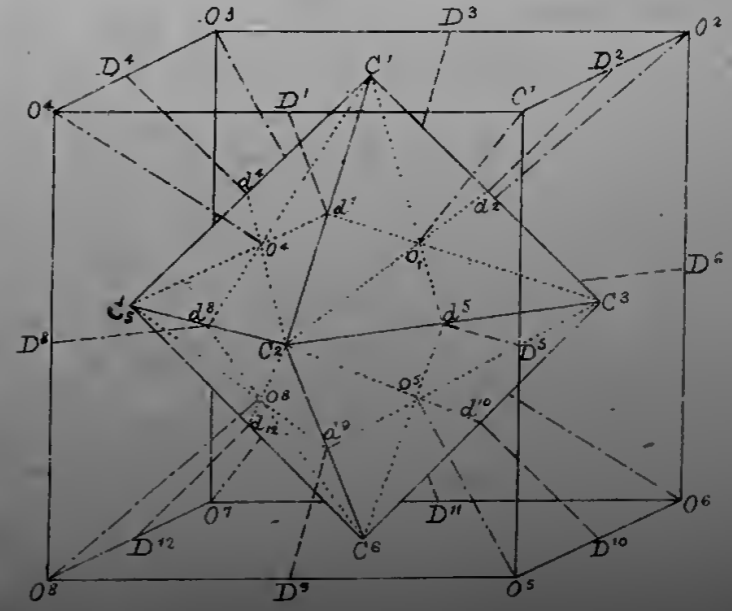




Fig. 15.

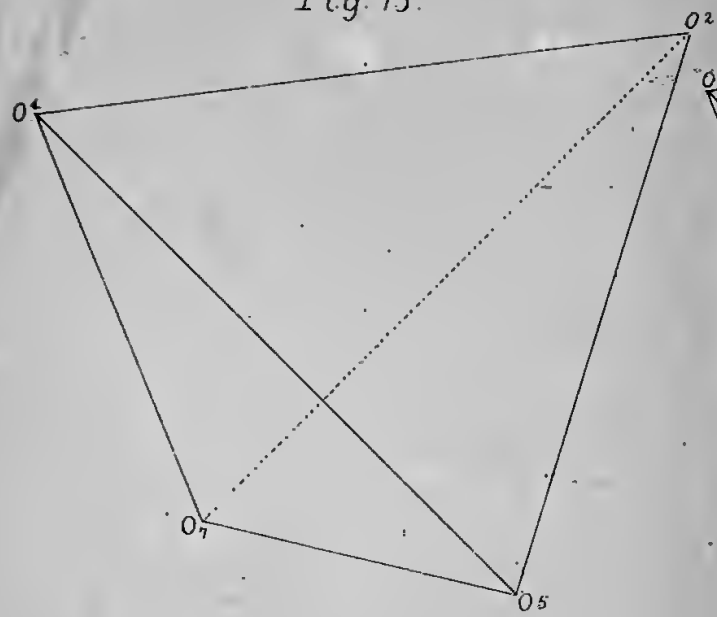


Fig. 16.

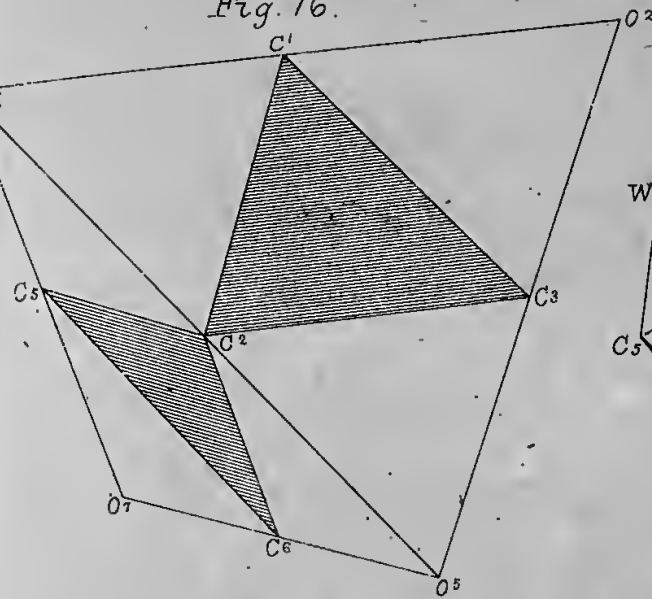


Fig. 17.

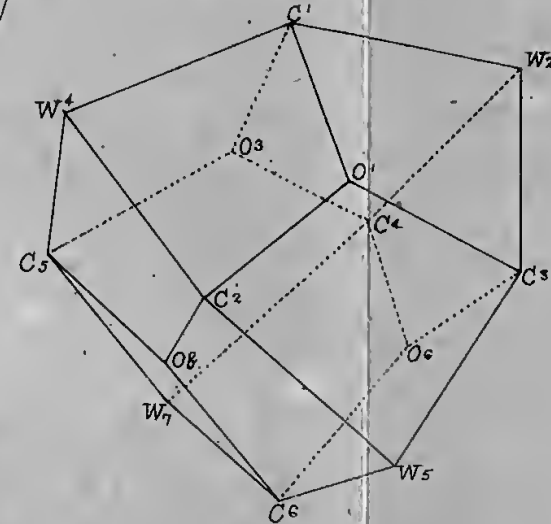


Fig. 18.

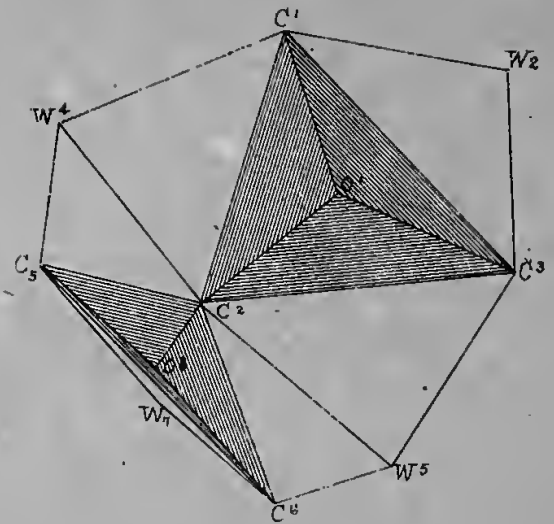


Fig. 19.

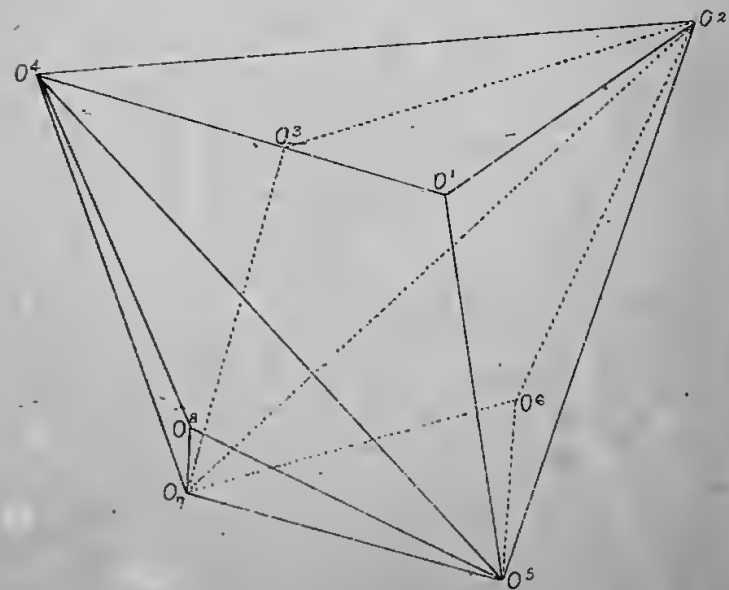


Fig. 20.

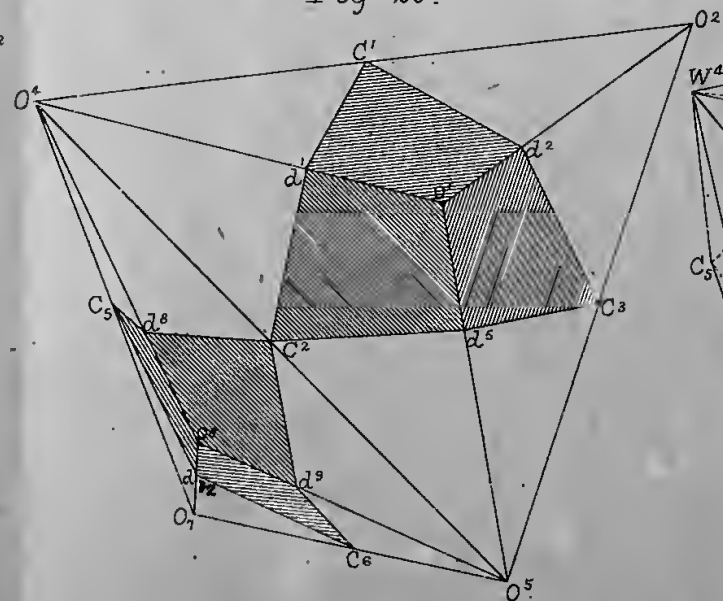


Fig. 21.

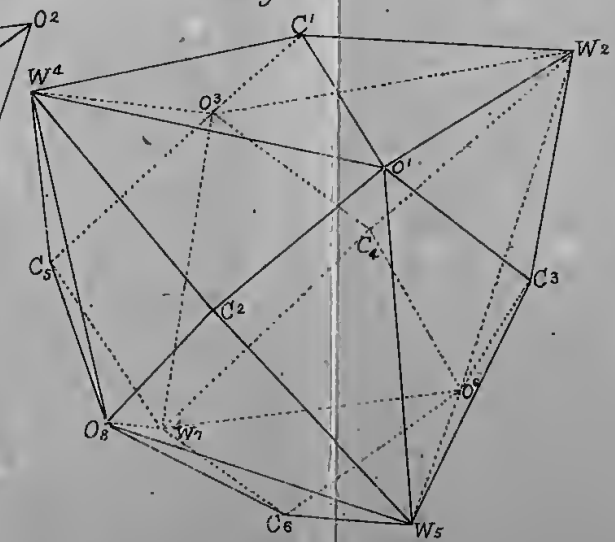


Fig. 22.

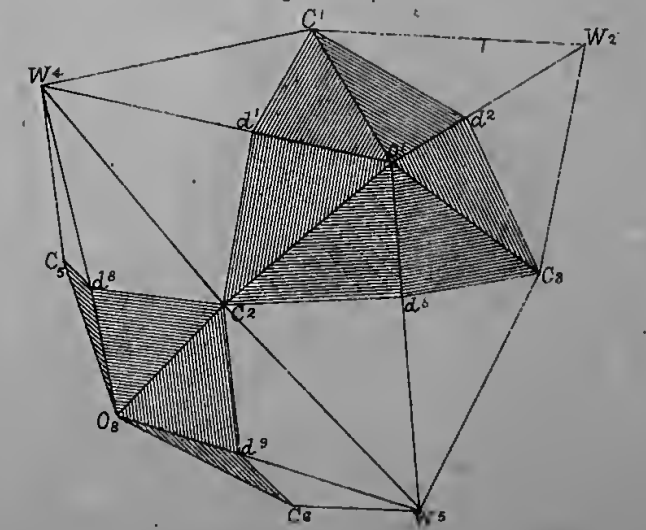


Fig. 23.

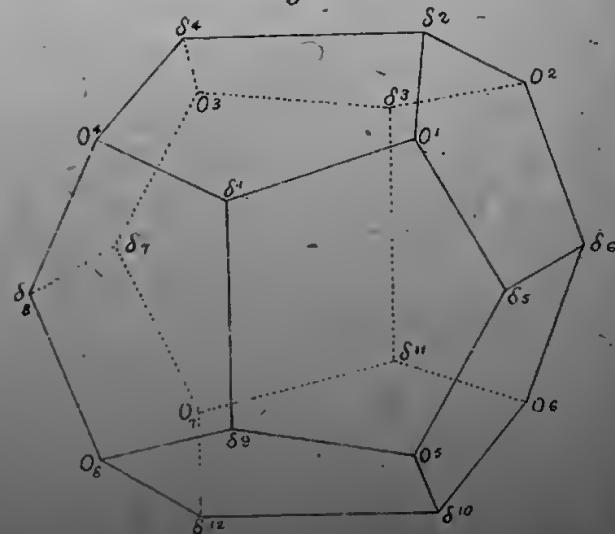


Fig. 24.

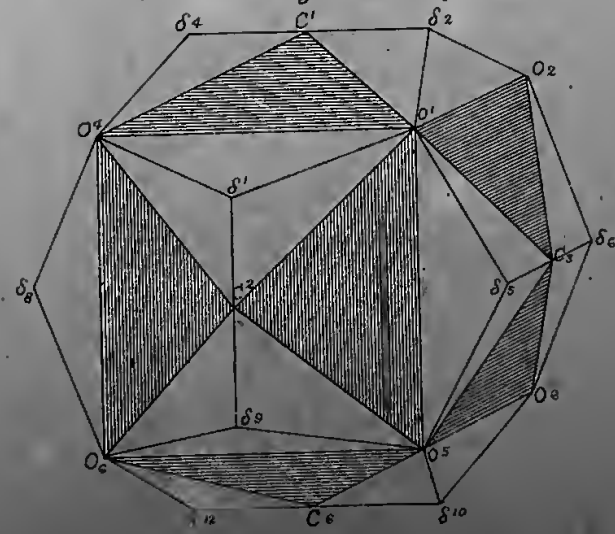


Fig. 25.

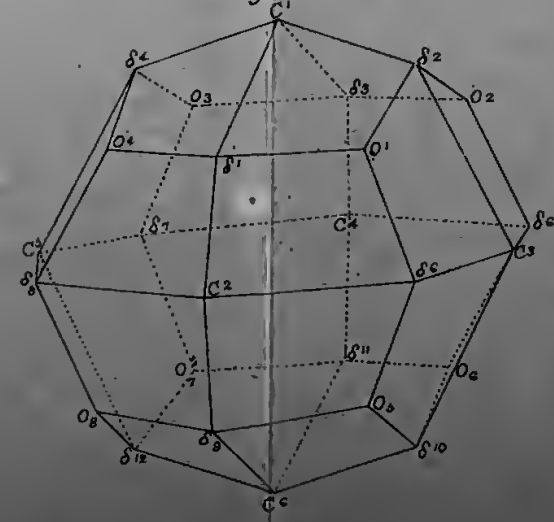


Fig. 26.

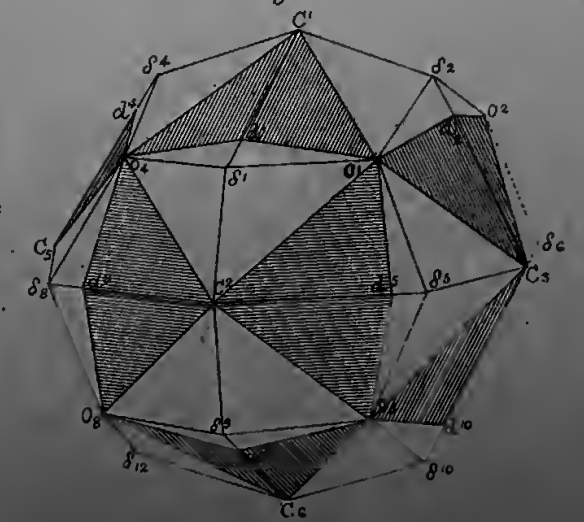




Fig. 27.

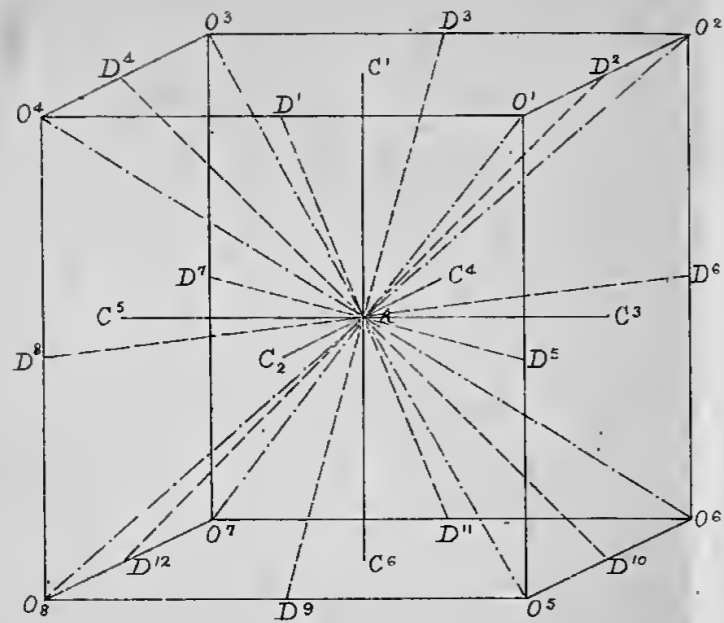


Fig. 28.

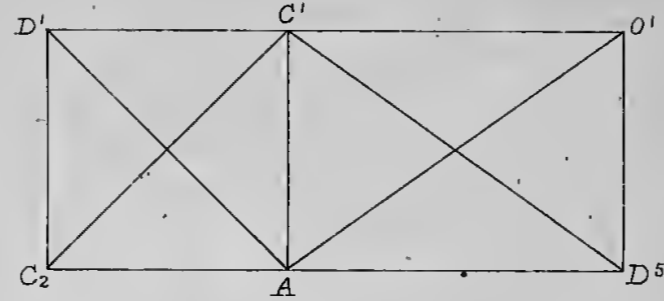


Fig. 29.

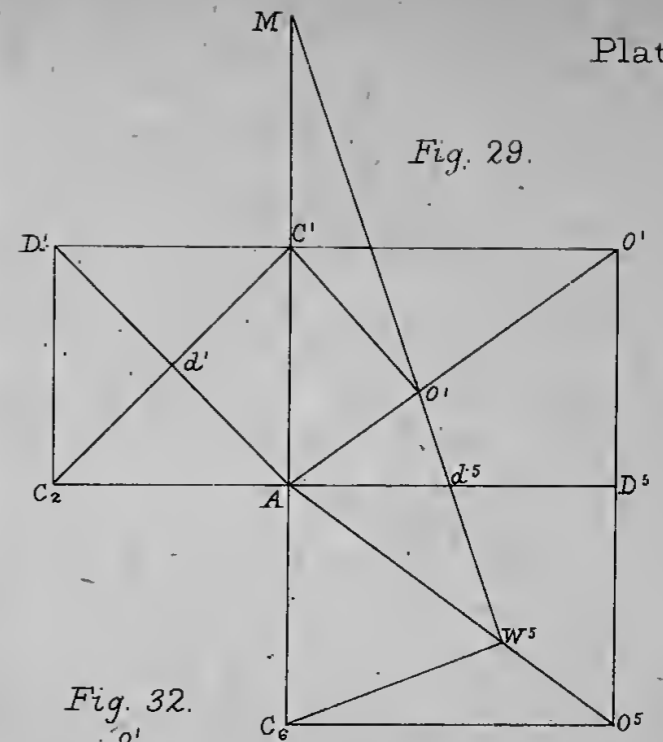


Fig. 31.

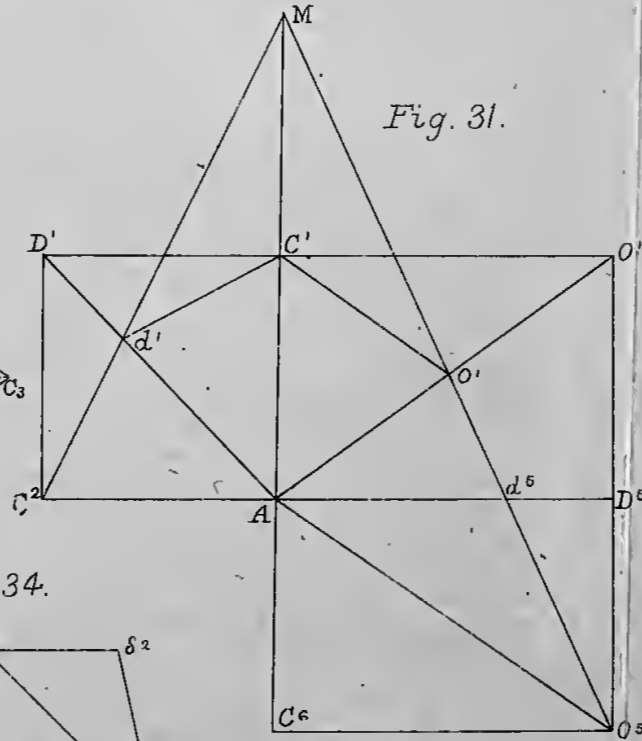


Fig. 30.

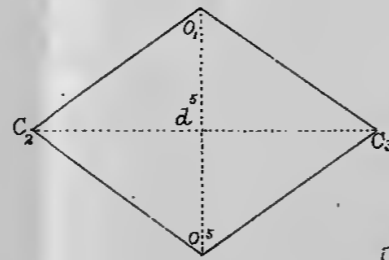


Fig. 32.

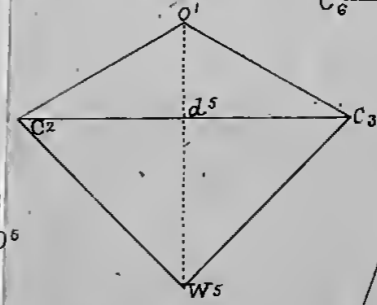


Fig. 33.

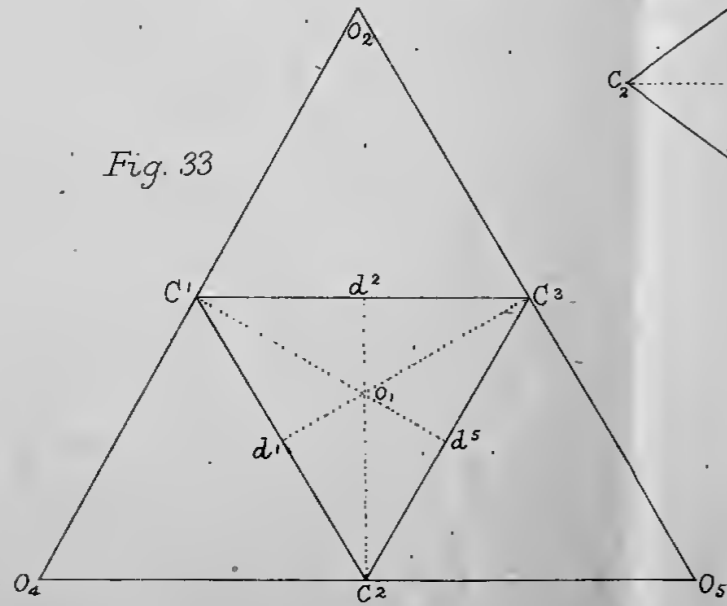


Fig. 34.

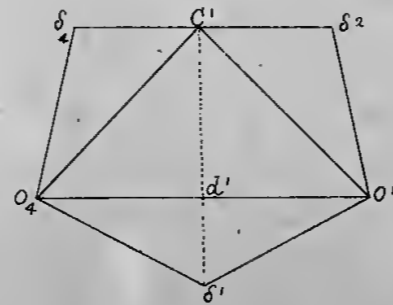


Fig. 37.

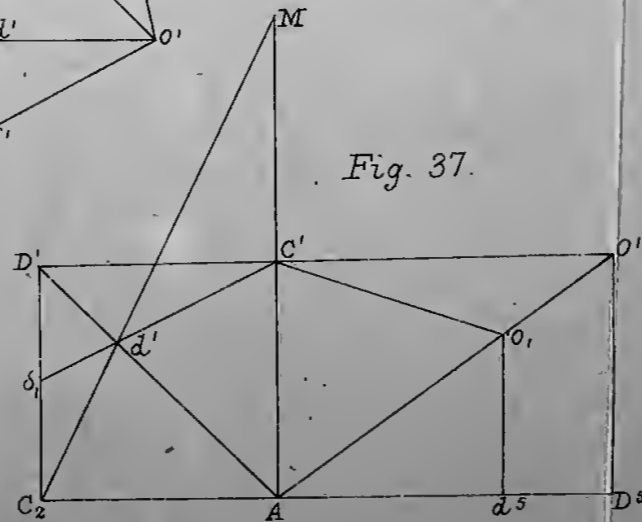


Fig. 35.

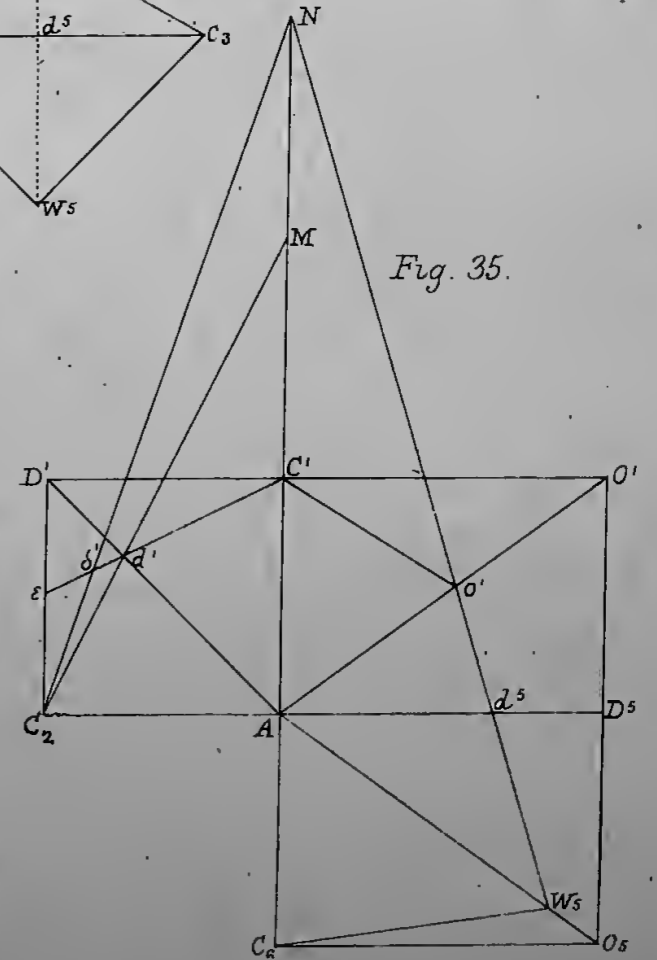


Fig. 36.

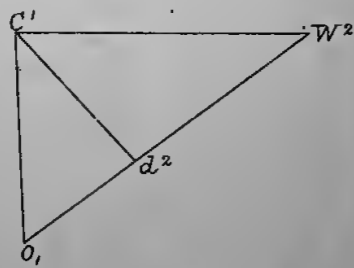


Fig. 38.

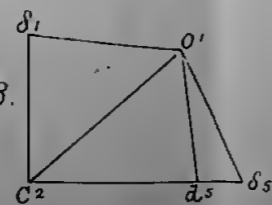


Fig. 39.

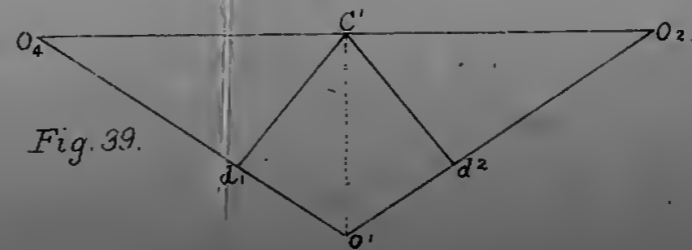




Fig. 27. *

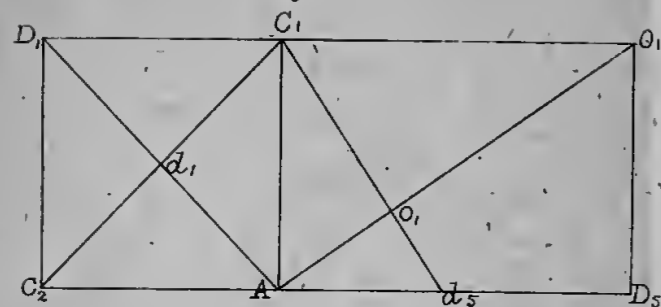


Fig. 28. *

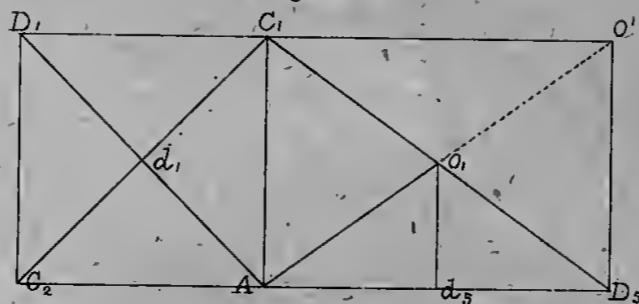


Fig. 29. *

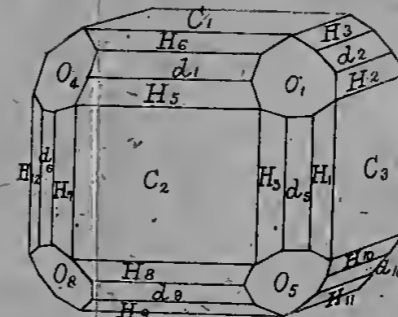


Fig. 33. *

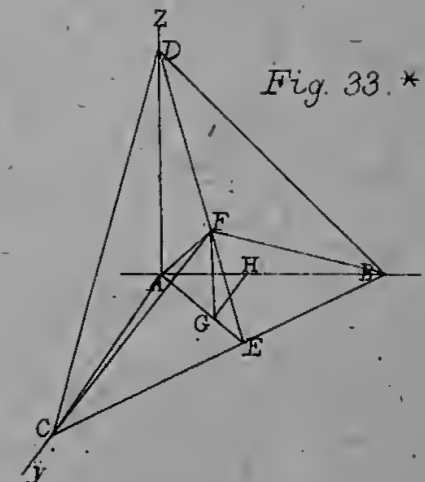


Fig. 30. *

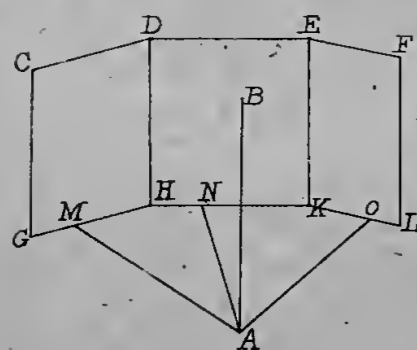


Fig. 31. *

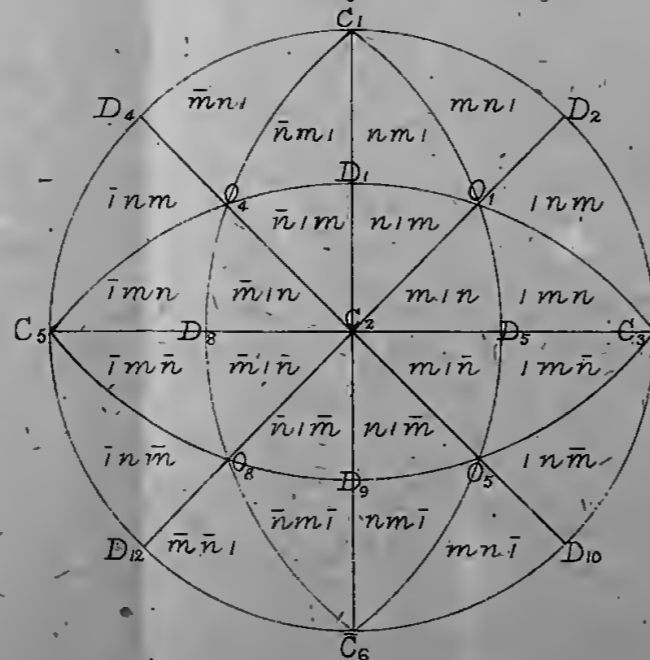


Fig. 32. *

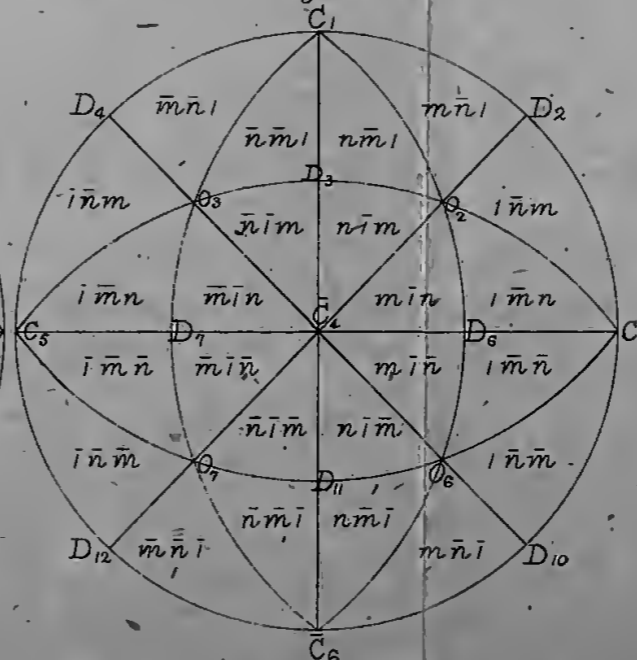


Fig. 34. *

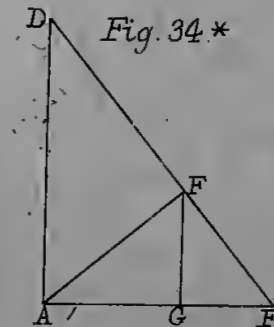


Fig. 35. *

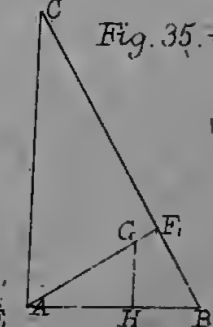


Fig. 36. *

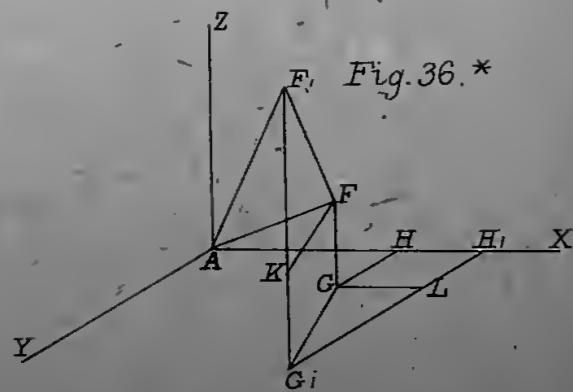


Fig. 38. *

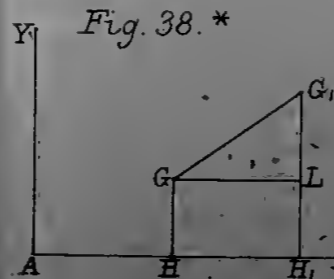


Fig. 37. *

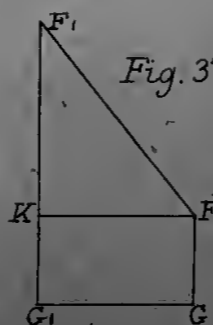


Fig. 39. *

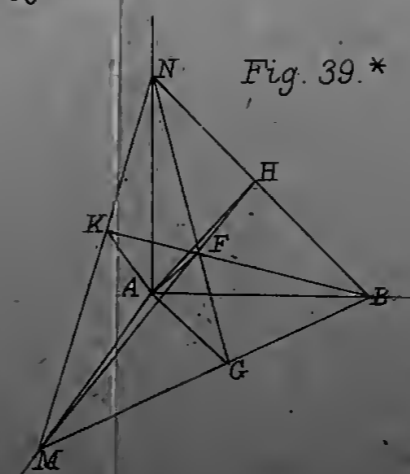


Fig. 40. *

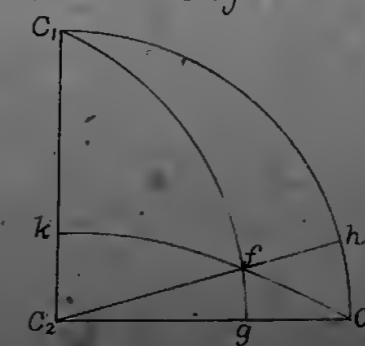




Fig. 41. *

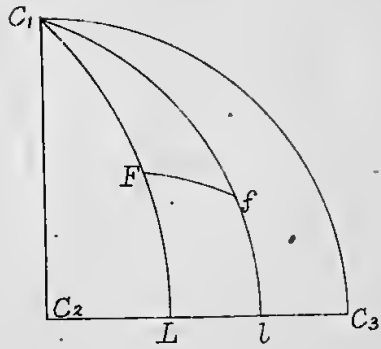


Fig. 42. *

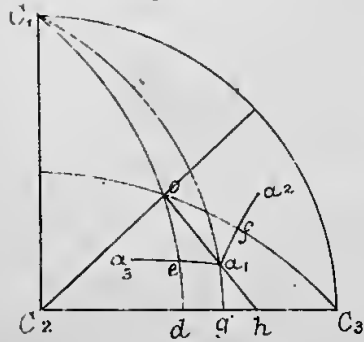


Fig. 43. *

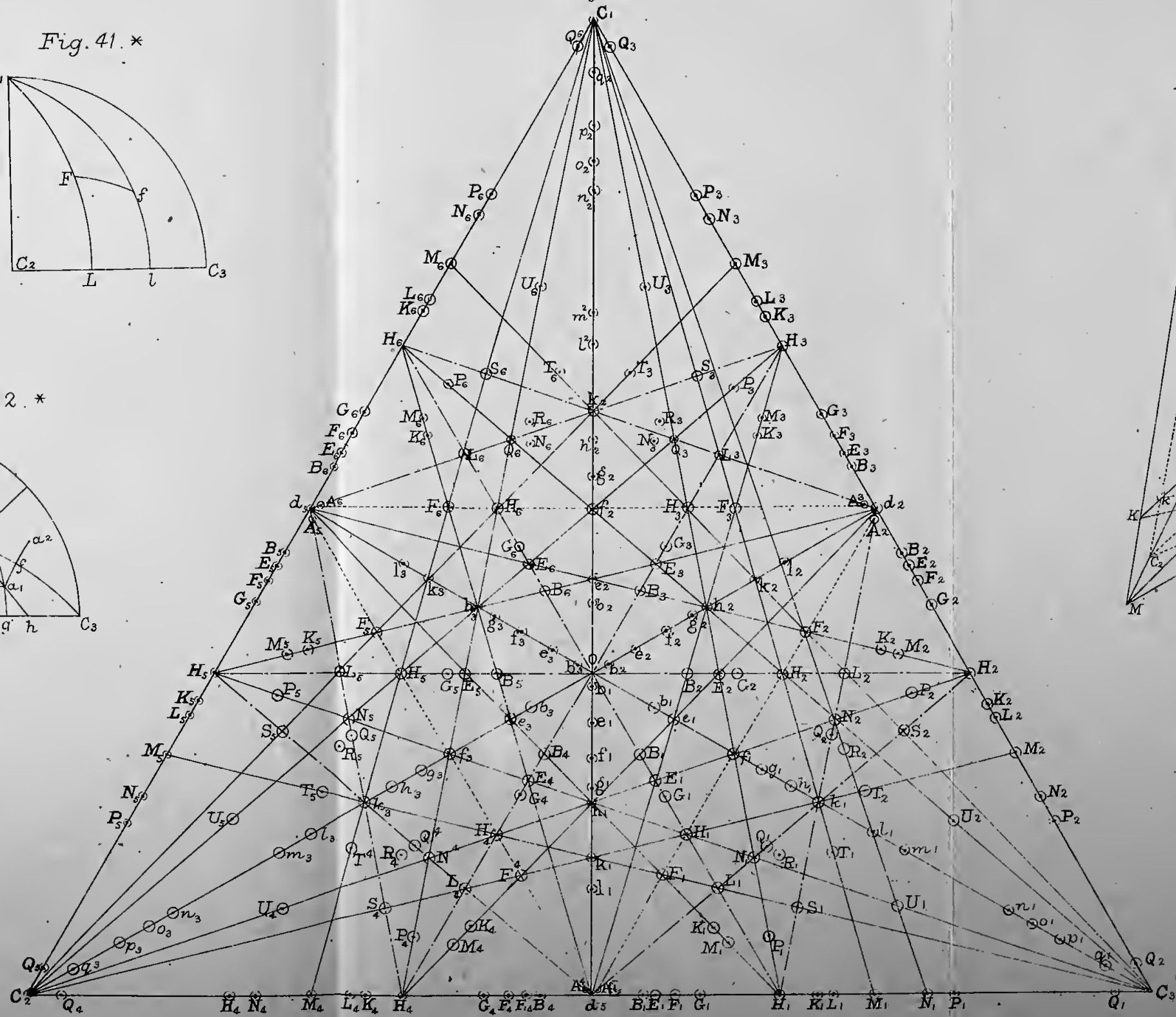


Fig. 44. *

